Vol. 55, No. 4 July, 1948

Psychological Review

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CONTENTS

PUBLISHED BI-MONTHLY BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STS., LANCASTER, PA.
AND 1515 MASSACHUSETTS AVE., N. W., WASHINGTON 5, D. C.

$5.50 volume $1.00 issue

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

Acceptance for mailing at the special rate of postage provided for in the Act of February 28, 1925, embodied in paragraph 4, Section 538, P. L. and R., authorized Jan. 8, 1948

PSYCHOLOGICAL REVIEW

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1948	55	By Subscription, $6.50						$1.00	—
		List price, Volumes 1 through 54							$265.50
		30% Discount							79.65
		Net price, Volumes 1 through 54							$185.85

Information about the Psychological Review: from 1894 to 1908 many numbers are out of print, as shown in the table. After 1908, only ten numbers are out of print. After 1926, no numbers are out of print.

The journal has been published with six numbers a year throughout its history.

Information about prices: the Psychological Review has the uniform price of $5.50 per volume and $1.00 per issue. For incomplete volumes, the price is $1.00 for each available number. For foreign postage, $.25 per volume should be added. The American Psychological Association gives the following discounts on orders for any one journal:

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AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

1515 Massachusetts Avenue, N. W. Washington 5, D. C.

VOL. 55, NO. 4 JULY, 1948

THE PSYCHOLOGICAL REVIEW

COGNITIVE MAPS IN RATS AND MEN [1]

BY EDWARD C. TOLMAN

University of California

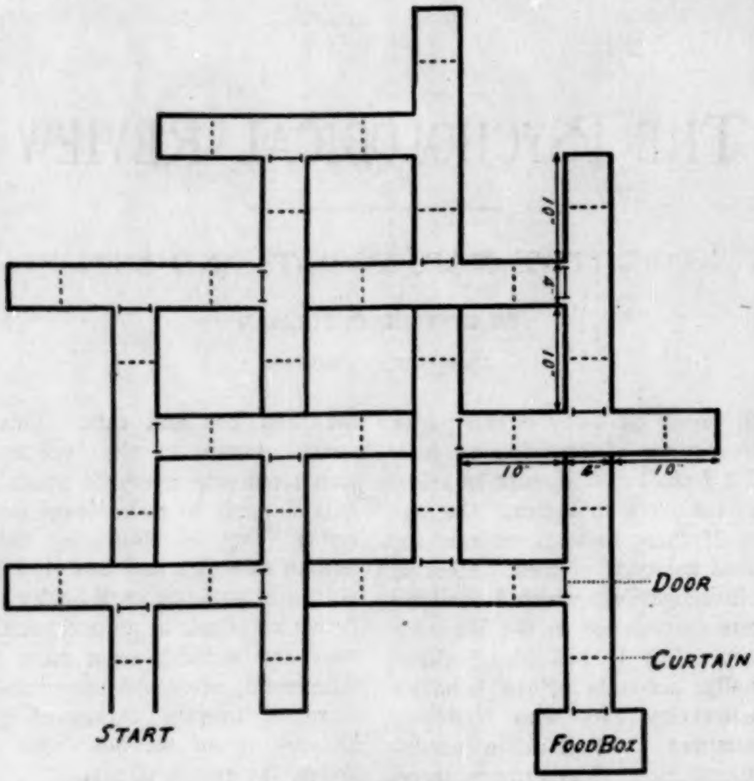
I shall devote the body of this paper to a description of experiments with rats. But I shall also attempt in a few words at the close to indicate the significance of these findings on rats for the clinical behavior of men. Most of the rat investigations, which I shall report, were carried out in the Berkeley laboratory. But I shall also include, occasionally, accounts of the behavior of non-Berkeley rats who obviously have misspent their lives in out-of-State laboratories. Furthermore, in reporting our Berkeley experiments I shall have to omit a very great many. The ones I *shall* talk about were carried out by graduate students (or underpaid research assistants) who, supposedly, got some of their ideas from me. And a few, though a very few, were even carried out by me myself.

Let me begin by presenting diagrams for a couple of typical mazes, an alley maze and an elevated maze. In the typical experiment a hungry rat is put at the entrance of the maze (alley or elevated), and wanders about through the various true path segments and blind alleys until he finally comes to the food box and eats. This is repeated (again in the typical experiment) one trial every 24 hours and the animal tends to make fewer and fewer errors (that is, blind-alley entrances) and to take less and less time between start and goal-box until finally he is entering no blinds at all and running in a very few seconds from start to goal. The results are usually presented in the form of average curves of blind-entrances, or of seconds from start to finish, for groups of rats.

All students agree as to the facts. They disagree, however, on theory and explanation.

(1) First, there is a school of animal psychologists which believes that the maze behavior of rats is a matter of mere simple stimulus-response connections. Learning, according to them, consists in the strengthening of some of these connections and in the weakening of others. According to this 'stimulus-response' school the rat in progressing down the maze is helplessly responding to a succession of external stimuli—sights, sounds, smells, pressures, etc. impinging upon his external sense organs—plus internal stimuli coming from the viscera and from the skeletal muscles. These external and internal stimuli call out the walkings, runnings, turnings, retracings, smellings, rearings, and the like which appear. The rat's central

[1] 34th Annual Faculty Research Lecture, delivered at the University of California, Berkeley, March 17, 1947. Presented also on March 26, 1947 as one in a series of lectures in Dynamic Psychology sponsored by the division of psychology of Western Reserve University, Cleveland, Ohio.



Plan of maze
14-Unit T-Alley Maze

FIG. 1

(From M. H. Elliott, The effect of change of reward on the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1928, 4, p. 20.)

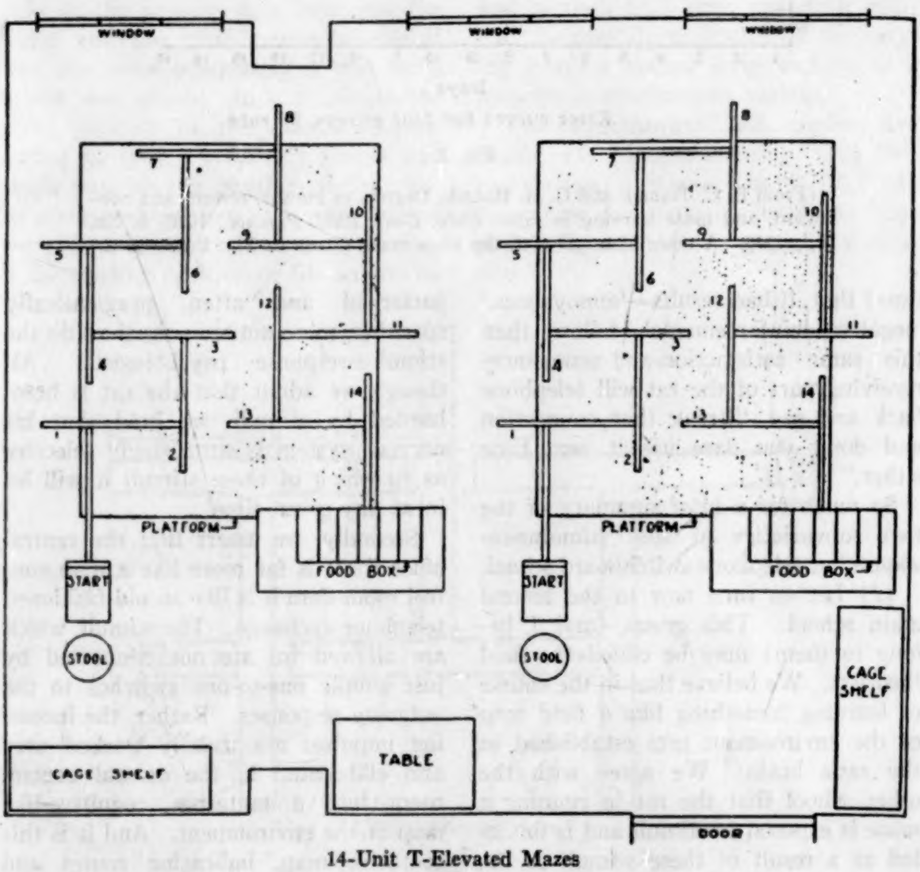
nervous system, according to this view, may be likened to a complicated telephone switchboard. There are the incoming calls from sense-organs and there are the outgoing messages to muscles. Before the learning of a specific maze, the connecting switches (synapses according to the physiologist) are closed in one set of ways and produce the primarily exploratory responses which appear in the early trials. *Learning*, according to this view, consists in the respective strengthening and weakening of various of these connections; those connections which result in the animal's going down the true path become relatively more open to the passage of nervous impulses, whereas those which lead him into the blinds become relatively less open.

It must be noted in addition, however, that this stimulus-response school divides further into two subgroups.

(a) There is a subgroup which holds that the mere mechanics involved in the running of a maze is such that the crucial stimuli from the maze get presented simultaneously with the correct responses more frequently than they do with any of the incorrect responses. Hence, just on a basis of this greater frequency, the neural connections between the crucial stimuli and the correct responses will tend, it is said, to

get strengthened at the expense of the incorrect connections.

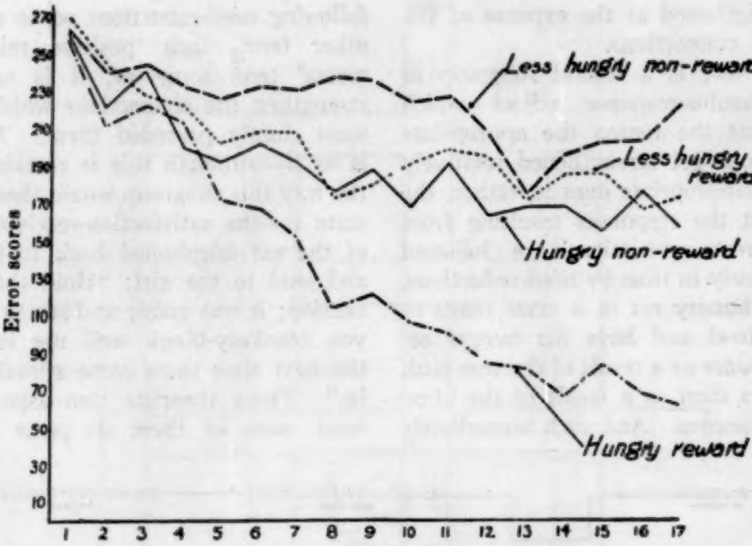
(b) There is a second subgroup in this stimulus-response school which holds that the reason the appropriate connections get strengthened relatively to the inappropriate ones is, rather, the fact that the responses resulting from the correct connections are followed more closely in time by need-reductions. Thus a hungry rat in a maze tends to get to food and have his hunger reduced *sooner* as a result of the true path responses than as a result of the blind alley responses. And such immediately following need-reductions or, to use another term, such 'positive reinforcements' tend somehow, it is said, to strengthen the connections which have most closely preceded them. Thus it is as if—although this is certainly not the way this subgroup would themselves state it—the satisfaction-receiving part of the rat telephoned back to Central and said to the girl: "Hold that connection; it was good; and see to it that you blankety-blank well use it again the next time these same stimuli come in." These theorists also assume (at least some of them do some of the



14-Unit T-Elevated Mazes

FIG. 2

(From C. H. Honzik, The sensory basis of maze learning in rats. *Compar. Psychol. Monogr.*, 1936, 13, No. 4, p. 4. These were two identical mazes placed side by side in the same room.)



Error curves for four groups, 36 rats.

FIG. 3

(From E. C. Tolman and C. H. Honzik, Degrees of hunger, reward and non-reward, and maze learning in rats. *Univ. Calif. Publ. Psychol.*, 1930, 4, No. 16, p. 246. A maze identical with the alley maze shown in Fig. 1 was used.)

time) that, if bad results—'annoyances,' 'negative reinforcements'—follow, then this same satisfaction-and-annoyance-receiving part of the rat will telephone back and say, "Break that connection and don't you dare use it next time either."

So much for a brief summary of the two subvarieties of the 'stimulus-response,' or telephone switchboard school.

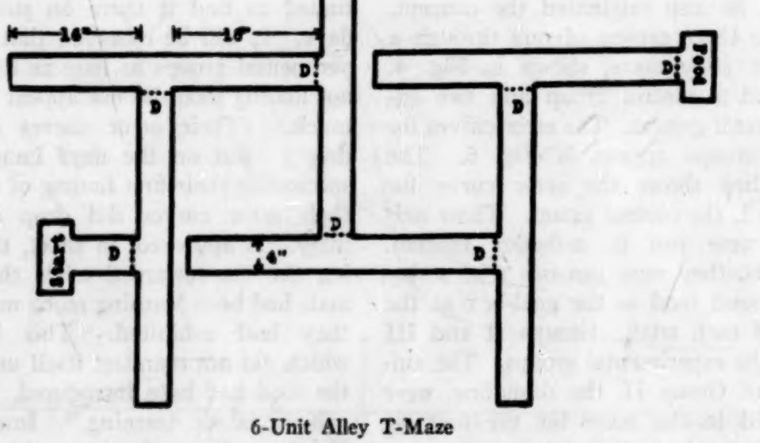
(2) Let us turn now to the second main school. This group (and I belong to them) may be called the field theorists. We believe that in the course of learning something like a field map of the environment gets established in the rat's brain. We agree with the other school that the rat in running a maze is exposed to stimuli and is finally led as a result of these stimuli to the responses which actually occur. We feel, however, that the intervening brain processes are more complicated, more patterned and often, pragmatically speaking, more autonomous than do the stimulus-response psychologists. Although we admit that the rat is bombarded by stimuli, we hold that his nervous system is surprisingly selective as to which of these stimuli it will let in at any given time.

Secondly, we assert that the central office itself is far more like a map control room than it is like an old-fashioned telephone exchange. The stimuli, which are allowed in, are not connected by just simple one-to-one switches to the outgoing responses. Rather, the incoming impulses are usually worked over and elaborated in the central control room into a tentative, cognitive-like map of the environment. And it is this tentative map, indicating routes and paths and environmental relationships, which finally determines what responses, if any, the animal will finally release.

Finally, I, personally, would hold further that it is also important to discover in how far these maps are relatively narrow and strip-like or relatively broad and comprehensive. Both strip-maps and comprehensive-maps may be either correct or incorrect in the sense that they may (or may not), when acted upon, lead successfully to the animal's goal. The differences between such strip maps and such comprehensive maps will appear only when the rat is later presented with some change within the given environment. Then, the narrower and more strip-like the original map, the less will it carry over successfully to the new problem; whereas, the wider and the more comprehensive it was, the more adequately it will serve in the new set-up. In a strip-map the given position of the animal is connected by only a relatively simple and single path to the position of the goal. In a comprehensive-map a wider arc of the environment is represented, so that, if the starting position of the animal be changed or variations in the specific routes be introduced, this wider map will allow the animal still to behave relatively correctly and to choose the appropriate new route.

But let us turn, now, to the actual experiments. The ones, out of many, which I have selected to report are simply ones which seem especially important in reinforcing the theoretical position I have been presenting. This position, I repeat, contains two assumptions: First, that learning consists not in stimulus-response connections but in the building up in the nervous system of sets which function like cognitive maps, and second, that such cognitive maps may be usefully characterized as varying from a narrow strip variety to a broader comprehensive variety.

The experiments fall under five heads: (1) "latent learning," (2) "vicarious trial and error" or "VTE," (3) "searching for the stimulus," (4) "hypotheses" and (5) "spatial orientation."



6-Unit Alley T-Maze

FIG. 4

(From H. C. Blodgett, The effect of the introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, 4, No. 8, p. 117.)

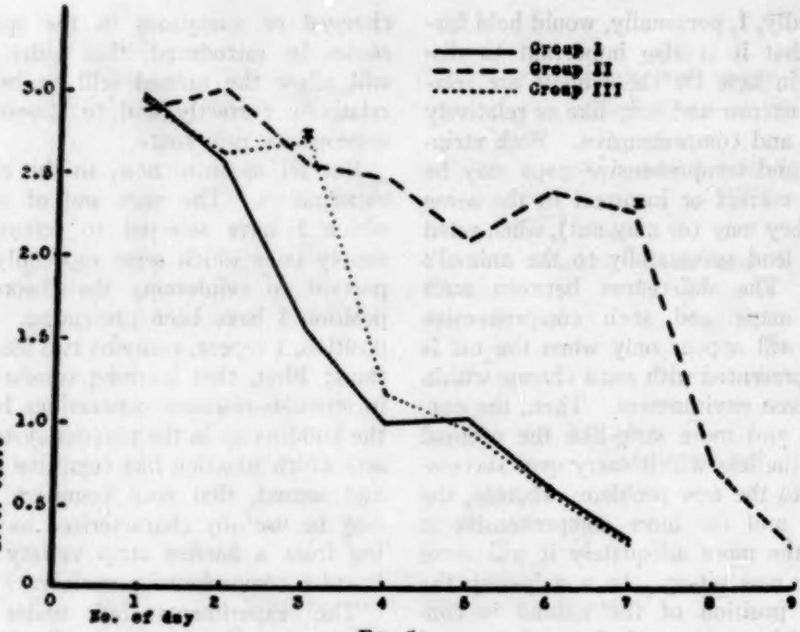


FIG. 5

(From H. C. Blodgett, The effect of the introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, 4, No. 8, p. 120.)

(1) *"Latent Learning" Experiments.* The first of the latent learning experiments was performed at Berkeley by Blodgett. It was published in 1929. Blodgett not only performed the experiments, he also originated the concept. He ran three groups of rats through a six-unit alley maze, shown in Fig. 4. He had a control group and two experimental groups. The error curves for these groups appear in Fig. 5. The solid line shows the error curve for Group I, the control group. These animals were run in orthodox fashion. That is, they were run one trial a day and found food in the goal-box at the end of each trial. Groups II and III were the experimental groups. The animals of Group II, the dash line, were not fed in the maze for the first six days but only in their home cages some two hours later. On the seventh day (indicated by the small cross) the rats found food at the end of the maze for the first time and continued to find it on subsequent days. The animals of Group III were treated similarly except that they first found food at the end of the maze on the third day and continued to find it there on subsequent days. It will be observed that the experimental groups as long as they were not finding food did not appear to learn much. (Their error curves did not drop.) But on the days immediately succeeding their first finding of the food their error curves did drop astoundingly. It appeared, in short, that during the non-rewarded trials these animals had been learning much more than they had exhibited. This learning, which did not manifest itself until after the food had been introduced, Blodgett called "latent learning." Interpreting these results anthropomorphically, we would say that as long as the animals were not getting any food at the end of the maze they continued to take their

time in going through it—they continued to enter many blinds. Once, however, they knew they were to get food, they demonstrated that during these preceding non-rewarded trials they had learned where many of the blinds were. They had been building up a 'map,' and could utilize the latter as soon as they were motivated to do so.

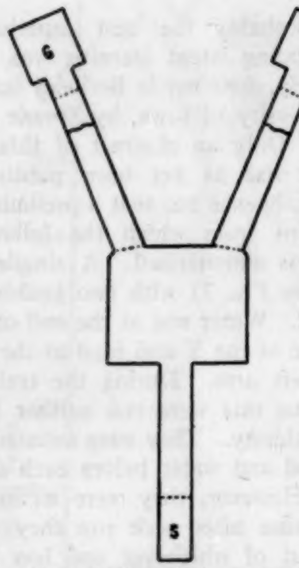
Honzik and myself repeated the experiments (or rather he did and I got some of the credit) with the 14-unit T-mazes shown in Fig. 1, and with larger groups of animals, and got similar results. The resulting curves are shown in Fig. 6. We used two control groups—one that never found food in the maze (HNR) and one that found it throughout (HR). The experimental group (HNR–R) found food at the end of the maze from the 11th day on and showed the same sort of a sudden drop.

But probably the best experiment demonstrating latent learning was, unfortunately, done not in Berkeley but at the University of Iowa, by Spence and Lippitt. Only an abstract of this experiment has as yet been published. However, Spence has sent a preliminary manuscript from which the following account is summarized. A simple Y-maze (see Fig. 7) with two goal-boxes was used. Water was at the end of the right arm of the Y and food at the end of the left arm. During the training period the rats were run neither hungry nor thirsty. They were satiated for both food and water before each day's trials. However, they were willing to run because after each run they were taken out of whichever end box they had got to and put into a living cage, with other animals in it. They were given four trials a day in this fashion

Error curves for HR, HNR, and HNR–R

FIG. 6

(From E. C. Tolman and C. H. Honzik, Introduction and removal of reward, and maze performance in rats. *Univ. Calif. Publ. Psychol.*, 1930, 4, No. 19, p. 267.)



Ground plan of the apparatus

FIG. 7

(Taken from K. W. Spence and R. Lippitt, An experimental test of the sign-gestalt theory of trial and error learning. *J. exper. Psychol.*, 1946, 36, p. 494. In this article they were describing another experiment but used the same maze.)

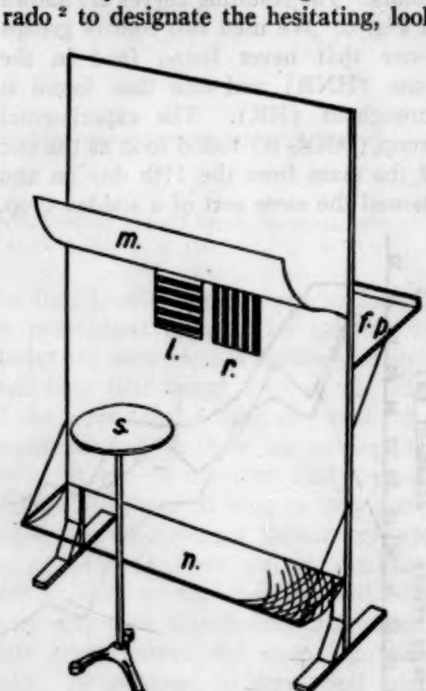
for seven days, two trials to the right and two to the left.

In the crucial test the animals were divided into two subgroups one made solely hungry and one solely thirsty. It was then found that on the first trial the hungry group went at once to the left, where the food had been, statistically more frequently than to the right; and the thirsty group went to the right, where the water had been, statistically more frequently than to the left. These results indicated that under the previous non-differential and very mild rewarding conditions of merely being returned to the home cages the animals had nevertheless been learning where the water was and where the food was. In short, they had acquired a cognitive map to the effect that food was to the left and water to the right, although during the acquisition of this map they had not exhibited any stimulus-response propensities to go more to the side which became later the side of the appropriate goal.

There have been numerous other latent learning experiments done in the Berkeley laboratory and elsewhere. In general, they have for the most part all confirmed the above sort of findings.

Let us turn now to the second group of experiments.

(2) *"Vicarious Trial and Error" or "VTE."* The term Vicarious Trial and Error (abbreviated as VTE) was invented by Prof. Muenzinger at Colorado [2] to designate the hesitating, look-



Apparatus Used for Testing Discrimination of Visual Patterns

FIG. 8

(From K. S. Lashley, The mechanism of vision. I. A method for rapid analyses of pattern-vision in the rat. *J. genet. Psychol.*, 1930, 37, p. 454.)

[2] *Vide:* K. F. Muenzinger, Vicarious trial and error at a point of choice: I. A general survey of its relation to learning efficiency. *J. genet. Psychol.*, 1938, 53, 75–86.

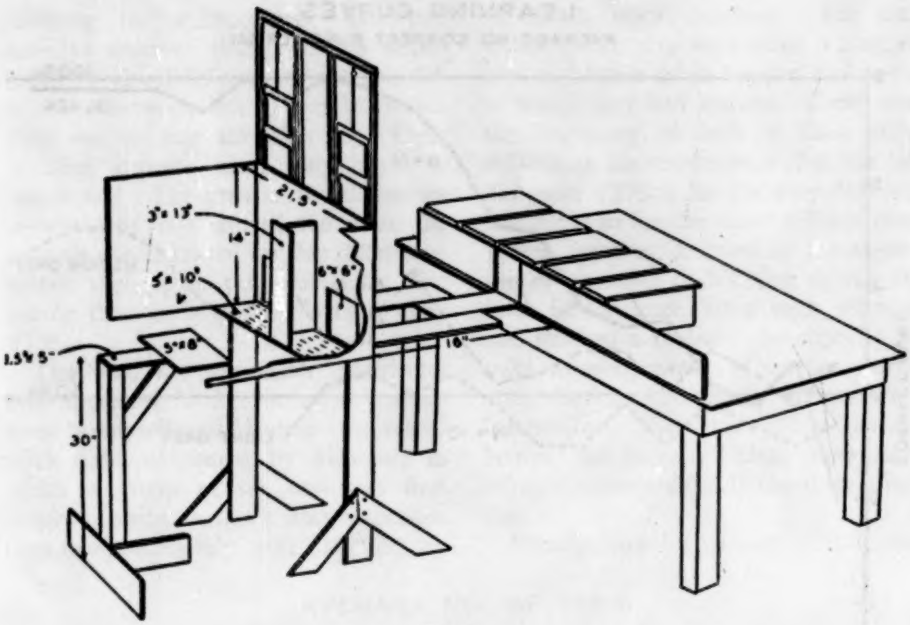


FIG. 9

(From E. C. Tolman, Prediction of vicarious trial and error by means of the schematic sowbug. PSYCHOL. REV., 1939, 46, p. 319.)

ing-back-and-forth, sort of behavior which rats can often be observed to indulge in at a choice-point before actually going one way or the other.

Quite a number of experiments upon VTEing have been carried out in our laboratory. I shall report only a few. In most of them what is called a discrimination set-up has been used. In one characteristic type of visual discrimination apparatus designed by Lashley (shown in Fig. 8) the animal is put on a jumping stand and faced with two doors which differ in some visual property say, as here shown, vertical stripes vs. horizontal stripes.

One of each such pair of visual stimuli is made always correct and the other wrong; and the two are interchanged from side to side in random fashion. The animal is required to learn, say, that the vertically striped door is always the correct one. If he jumps to it, the door falls open and he gets to food on a platform behind. If, on the other hand, he jumps incorrectly, he finds the door locked and falls into a net some two feet below from which he is picked up and started over again.

Using a similar set-up (see Fig. 9), but with landing platforms in front of the doors so that if the rat chose incorrectly he could jump back again and start over, I found that when the choice was an easy one, say between a white door and a black door, the animals not only learned sooner but also did more VTEing than when the choice was difficult, say between a white door and a gray door (see Fig. 10). It appeared further (see Fig. 11) that the VTEing began to appear just as (or just before) the rats began to learn. After the learning had become established, however, the VTE's began to go down. Further, in a study of individual dif-

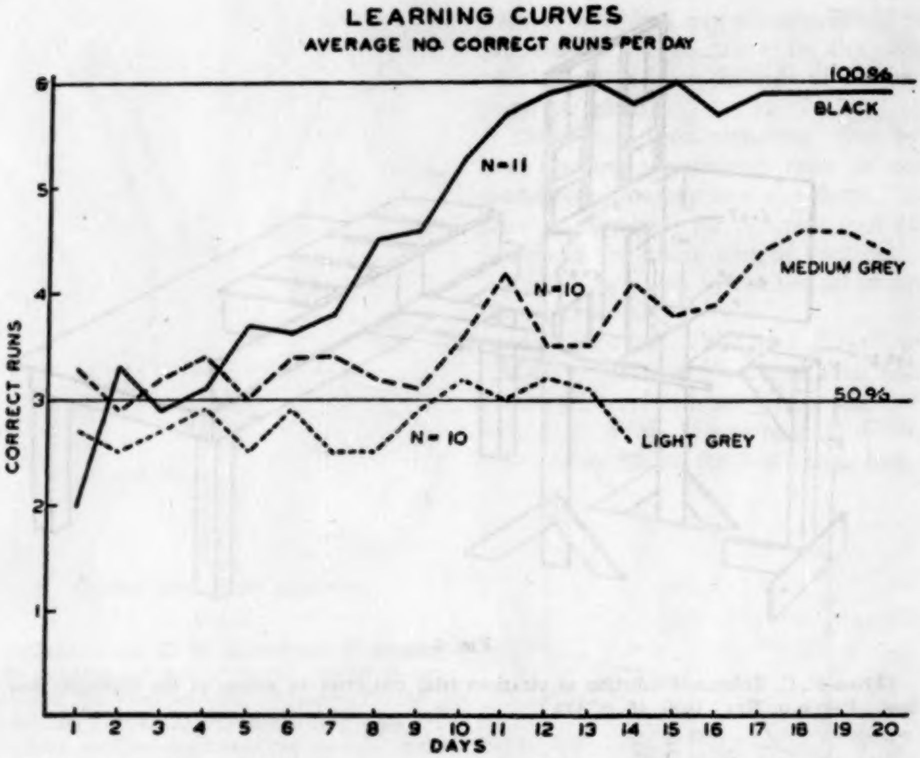


FIG. 10

(From E. C. Tolman, Prediction of vicarious trial and error by means of the schematic sowbug. PSYCHOL. REV., 1939, 46, p. 319.)

ferences by myself, Geier and Levin[8] (actually done by Geier and Levin) using this same visual discrimination apparatus, it was found that with one and the same difficulty of problem the smarter animal did the more VTEing.

To sum up, in *visual discrimination* experiments the better the learning, the more the VTE's. But this seems contrary to what we would perhaps have expected. We ourselves would expect to do more VTEing, more sampling of the two stimuli, when it is difficult to choose between them than when it is easy.

[8] F. M. Geier, M. Levin & E. C. Tolman, Individual differences in emotionality, hypothesis formation, vicarious trial and error and visual discrimination learning in rats. *Compar. Psychol. Monogr.*, 1941, 17, No. 3.

What is the explanation? The answer lies, I believe, in the fact that the manner in which we set the visual discrimination problems for the rats and the manner in which we set similar problems for ourselves are different. *We* already have our 'instructions.' We know beforehand what it is we are to do. We are told, or we tell ourselves, that it is the lighter of the two grays, the heavier of the two weights, or the like, which is to be chosen. In such a setting we do more sampling, more VTEing, when the stimulus-difference is small. But for the rats the usual problem in a discrimination apparatus is quite different. They do not know what is wanted of them. The major part of their learning in most such experiments seems to consist in their dis-

covering the instructions. The rats have to discover that it is the differences in visual brightness, not the differences between left and right, which they are to pay attention to. Their VTEing appears when they begin to 'catch on.' The greater the difference between the two stimuli the more the animals are attracted by this difference. Hence the sooner they catch on, and during this catching on, the more they VTE.

That this is a reasonable interpretation appeared further, from an experiment by myself and Minium (the actual work done, of course, by Minium) in which a group of six rats was first taught a white vs. black discrimination, then two successively more difficult gray vs. black discriminations. For each difficulty the rats were given a long series of further trials beyond the points at which they had learned. Comparing the beginning of each of these three difficulties the results were that the rats did more VTEing for the easy discriminations than for the more difficult ones. When, however, it came to a comparison of amounts of VTEing during the final performance after each learning had reached a plateau, the opposite results were obtained. In other words, after the rats had finally divined their instructions, then they, like human beings, did more VTEing, more sampling, the more difficult the discrimination.

Finally, now let us note that it was

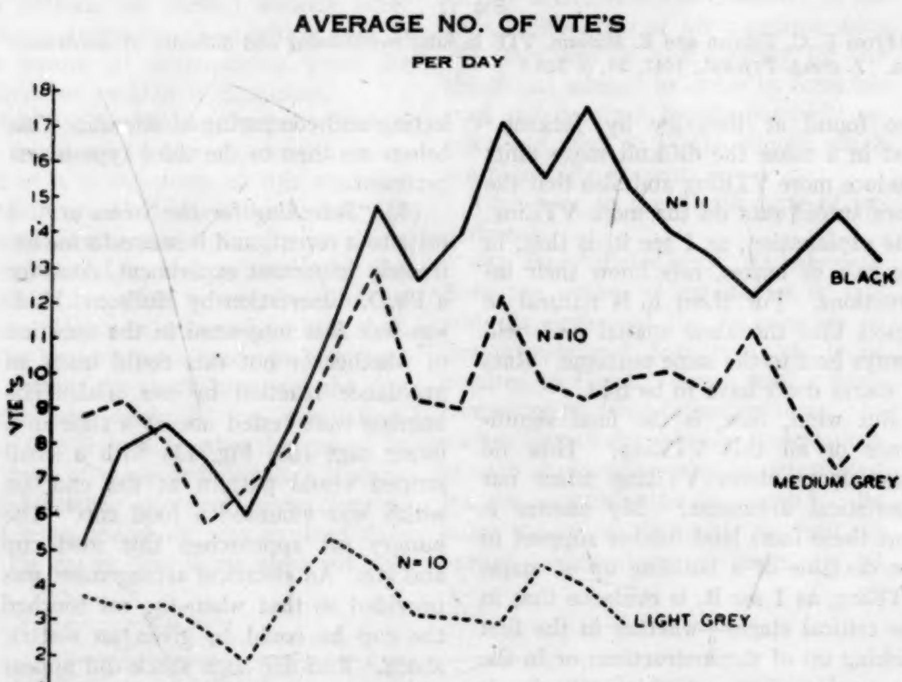


FIG. 11

(From E. C. Tolman, Prediction of vicarious trial and error by means of the schematic sowbug. PSYCHOL. REV., 1939, 46, p. 320.)

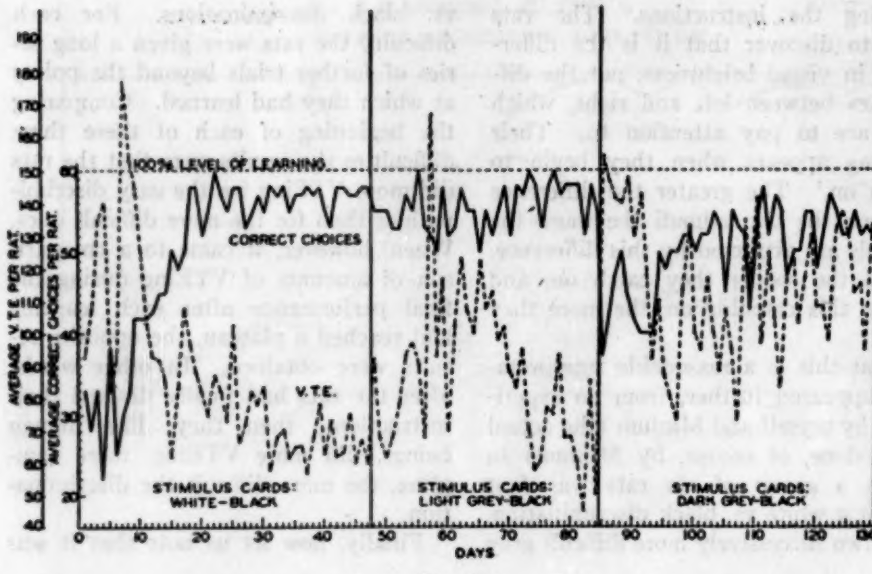


FIG. 12

(From E. C. Tolman and E. Minium, VTE in rats: overlearning and difficulty of discrimination. *J. comp. Psychol.*, 1942, 34, p. 303.)

also found at Berkeley by Jackson[4] that in a maze the difficult maze units produce more VTEing and also that the more stupid rats do the more VTEing. The explanation, as I see it, is that, in the case of mazes, rats know their instructions. For them it is natural to expect that the same spatial path will always lead to the same outcome. Rats in mazes don't have to be told.

But what, now, is the final significance of all this VTEing? How do these facts about VTEing affect our theoretical argument? My answer is that these facts lend further support to the doctrine of a building up of maps. VTEing, as I see it, is evidence that in the critical stages—whether in the first picking up of the instructions or in the later making sure of which stimulus is which—the animal's activity is not just one of responding passively to discrete stimuli, but rather one of the active selecting and comparing of stimuli. This brings me then to the third type of experiment.

(3) *"Searching for the Stimulus."* I refer to a recent, and it seems to me extremely important experiment, done for a Ph.D. dissertation by Hudson. Hudson was first interested in the question of whether or not rats could learn an avoidance reaction in one trial. His animals were tested one at a time in a living cage (see Fig. 13) with a small striped visual pattern at the end, on which was mounted a food cup. The hungry rat approached this food cup and ate. An electrical arrangement was provided so that when the rat touched the cup he could be given an electric shock. And one such shock did appear to be enough. For when the rat was replaced in this same cage days or even weeks afterwards, he usually demonstrated immediately strong avoidance reactions to the visual pattern. The animal withdrew from that end of the

[4] L. L. Jackson, V. T. E. on an elevated maze. *J. comp. Psychol.*, 1943, 36, 99–107.

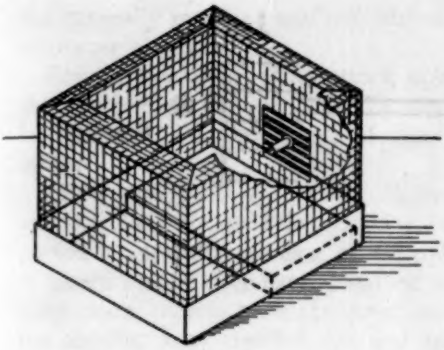


FIG. 13

(From Bradford Hudson. Ph.D. Thesis: 'One trial learning: A study of the avoidance behavior of the rat.' On deposit in the Library of the University of California, Berkeley, California.)

cage, or piled up sawdust and covered the pattern, or showed various other amusing responses all of which were in the nature of withdrawing from the pattern or making it disappear.

But the particular finding which I am interested in now appeared as a result of a modification of this standard procedure. Hudson noticed that the animals, anthropomorphically speaking, often seemed to look around *after* the shock to see what it was that had hit them. Hence it occurred to him that, if the pattern were made to disappear the instant the shock occurred, the rats might not establish the association. And this indeed is what happened in the case of many individuals. Hudson added further electrical connections so that when the shock was received during the eating, the lights went out, the pattern and the food cup dropped out of sight, and the lights came on again all within the matter of a second. When such animals were again put in the cage 24 hours later, a large percentage showed no avoidance of the pattern. Or to quote Hudson's own words:

"Learning what object to avoid . . . may occur exclusively during the period *after* the shock. For if the object from which the shock was actually received is removed at the moment of the shock, a significant number of animals fail to learn to avoid it, some selecting other features in the environment for avoidance, and others avoiding nothing."

In other words, I feel that this experiment reinforces the notion of the largely active selective character in the rat's building up of his cognitive map. He often has to look actively for the significant stimuli in order to form his map and does not merely passively receive and react to all the stimuli which are physically present.

Turn now to the fourth type of experiment.

(4) *The "Hypothesis" Experiments.* Both the notion of hypotheses in rats and the design of the experiments to demonstrate such hypotheses are to be credited to Krech. Krech used a four-compartment discrimination-box. In such a four-choice box the correct door at each choice-point may be determined by the experimenter in terms of its being lighted or dark, left or right, or various combinations of these. If all

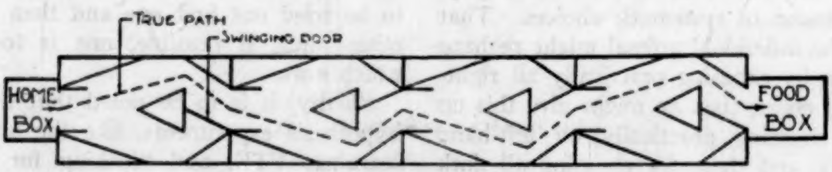
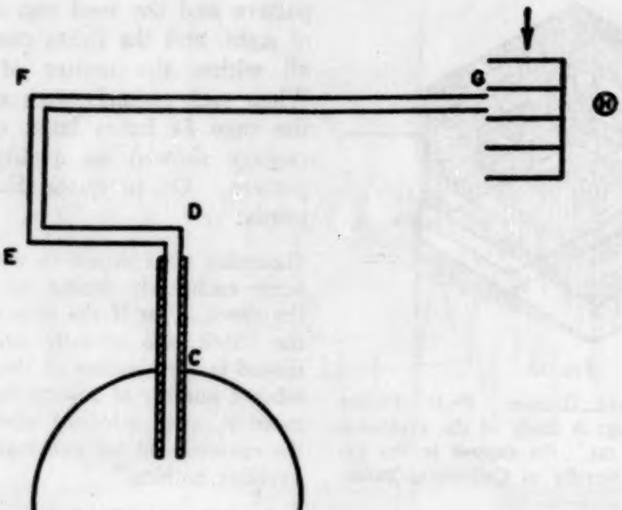


FIG. 14

(From I. Krechevsky (Now D. Krech), The genesis of "hypotheses" in rats. *Univ. Calif. Publ. Psychol.*, 1932, 6, No. 4, p. 46.)



Apparatus used in preliminary training

FIG. 15

(From E. C. Tolman, B. F. Ritchie and D. Kalish, Studies in spatial learning. I. Orientation and the short-cut. *J. exp. Psychol.*, 1946, 36, p. 16.)

possibilities are randomized for the 40 choices made in 10 runs of each day's test, the problem could be made insoluble.

When this was done, Krech found that the individual rat went through a succession of systematic choices. That is, the individual animal might perhaps begin by choosing practically all right-hand doors, then he might give this up for choosing practically all left-hand doors, and then, for choosing all dark doors, and so on. These relatively persistent, and well-above-chance systematic types of choice Krech called "hypotheses." In using this term he obviously did not mean to imply verbal processes in the rat but merely referred to what I have been calling cognitive maps which, it appears from his experiments, get set up in a tentative fashion to be tried out first one and then another until, if possible, one is found which works.

Finally, it is to be noted that these hypothesis experiments, like the latent learning, VTE, and "looking for the stimulus" experiments, do not, as such, throw light upon the widths of the maps which are picked up but do indicate

the generally map-like and self-initiated character of learning.

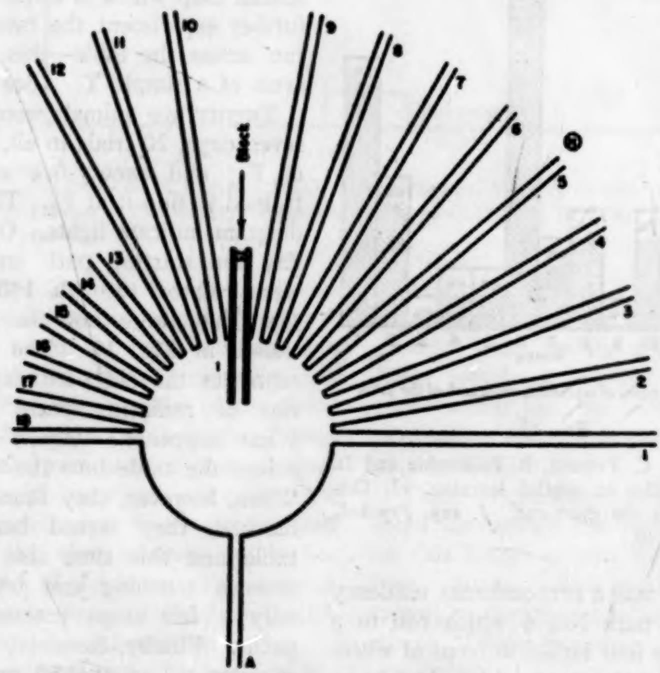
For the beginning of an attack upon the problem of the width of the maps let me turn to the last group of experiments.

(5) *"Spatial Orientation" Experiments.* As early as 1929, Lashley reported incidentally the case of a couple of his rats who, after having learned an alley maze, pushed back the cover near the starting box, climbed out and ran directly across the top to the goal-box where they climbed down in again and ate. Other investigators have reported related findings. All such observations suggest that rats really develop wider spatial maps which include more than the mere trained-on specific paths. In the experiments now to be reported this possibility has been subjected to further examination.

In the first experiment, Tolman, Ritchie and Kalish (actually Ritchie and Kalish) used the set-up shown in Fig. 15.

This was an elevated maze. The animals ran from A across the open circular table through CD (which had alley walls) and finally to G, the food box. H was a light which shone directly down the path from G to F. After four nights, three trials per night, in which the rats learned to run directly and without hesitation from A to G, the apparatus was changed to the sun-burst shown in Fig. 16. The starting path and the table remained the same but a series of radiating paths was added.

The animals were again started at A

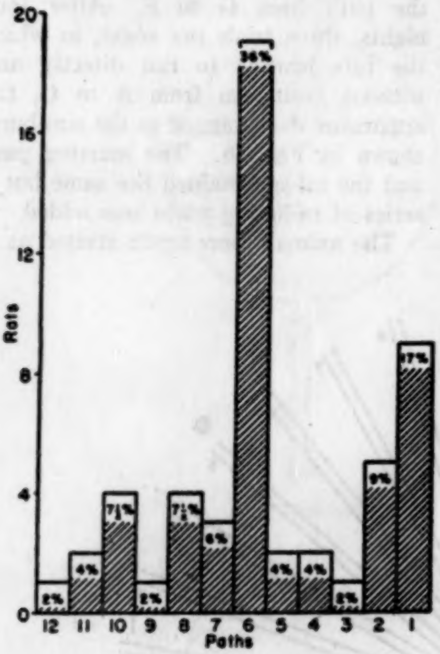


Apparatus used in the test trial

FIG. 16

(From E. C. Tolman, B. F. Ritchie and D. Kalish, Studies in spatial learning. I. Orientation and short-cut. *J. exp. Psychol.*, 1946, 36, p. 17.)

and ran across the circular table into the alley and found themselves blocked. They then returned onto the table and began exploring practically all the radiating paths. After going out a few inches only on any one path, each rat finally chose to run all the way out on one. The percentages of rats finally choosing each of the long paths from 1 to 12 are shown in Fig. 17. It appears



Numbers of rats which chose each of the paths

FIG. 17

(From E. C. Tolman, B. F. Ritchie and D. Kalish, Studies in spatial learning. I. Orientation and the short-cut. *J. exp. Psychol.*, 1946, 36, p. 19.)

that there was a preponderant tendency to choose path No. 6 which ran to a point some four inches in front of where the entrance to the food-box had been. The only other path chosen with any appreciable frequency was No. 1—that is, the path which pointed perpendicularly to the food-side of the room.

These results seem to indicate that the rats in this experiment had learned not only to run rapidly down the original roundabout route but also, when this was blocked and radiating paths presented, to select one pointing rather directly towards the point where the food had been or else at least to select a path running perpendicularly to the food-side of the room.

As a result of their original training, the rats had, it would seem, acquired not merely a strip-map to the effect that the original specifically trained-on path led to food but, rather, a wider comprehensive map to the effect that food was located in such and such a direction in the room.

Consider now a further experiment done by Ritchie alone. This experiment tested still further the breadth of the spatial map which is acquired. In this further experiment the rats were again run across the table—this time to the arms of a simple T. (See Fig. 18.)

Twenty-five animals were trained for seven days, 20 trials in all, to find food at F_1; and twenty-five animals were trained to find it at F_2. The L's in the diagram indicate lights. On the eighth day the starting path and table top were rotated through 180 degrees so that they were now in the position shown in Fig. 19. The dotted lines represent the old position. And a series of radiating paths was added. What happened? Again the rats ran across the table into the central alley. When, however, they found themselves blocked, they turned back onto the table and this time also spent many seconds touching and trying out for only a few steps practically all the paths. Finally, however, within seven minutes, 42 of the 50 rats chose one path and ran all the way out on it. The paths finally chosen by the 19 of these animals that had been fed at F_1 and by the 23 that had been fed at F_2 are shown in Fig. 20.

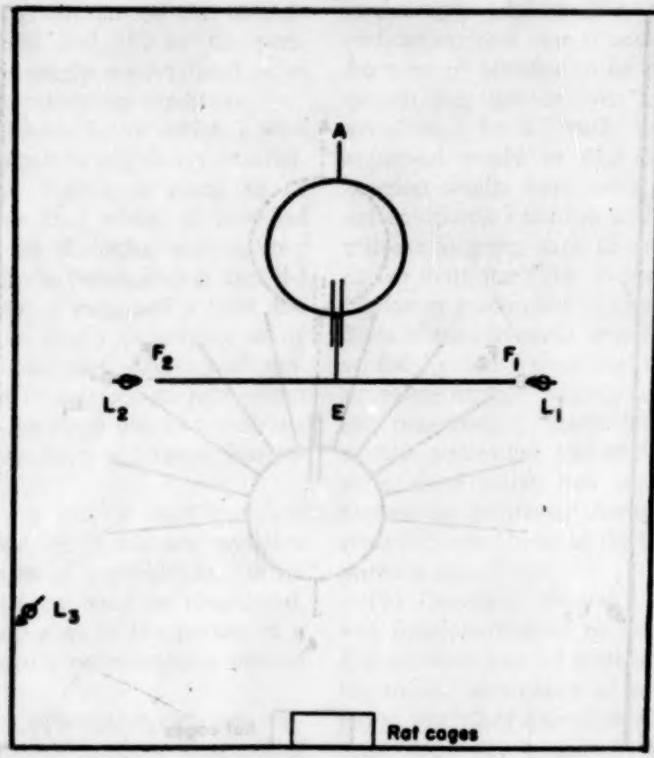


FIG. 18

(From B. F. Ritchie. Ph.D. Thesis: 'Spatial learning in rats.' On deposit in the Library of the University of California, Berkeley, California.)

This time the rats tended to choose, not the paths which pointed directly to the spots where the food had been, but rather paths which ran perpendicularly to the corresponding sides of the room. The spatial maps of these rats, when the animals were started from the opposite side of the room, were thus not completely adequate to the precise goal positions but were adequate as to the correct sides of the room. The maps of these animals were, in short, not altogether strip-like and narrow.

This completes my report of experiments. There were the *latent learning experiments*, the *VTE experiments*, the *searching for the stimulus experiment*, the *hypothesis experiments*, and these last *spatial orientation experiments*.

And now, at last, I come to the humanly significant and exciting problem: namely, what are the conditions which favor narrow strip-maps and what are those which tend to favor broad comprehensive maps not only in rats but also in men?

There is considerable evidence scattered throughout the literature bearing on this question both for rats and for men. Some of this evidence was obtained in Berkeley and some of it elsewhere. I have not time to present it in any detail. I can merely summarize it by saying that narrow strip maps rather than broad comprehensive maps seem to be induced: (1) by a damaged brain, (2) by an inadequate array of environmentally presented cues, (3) by an

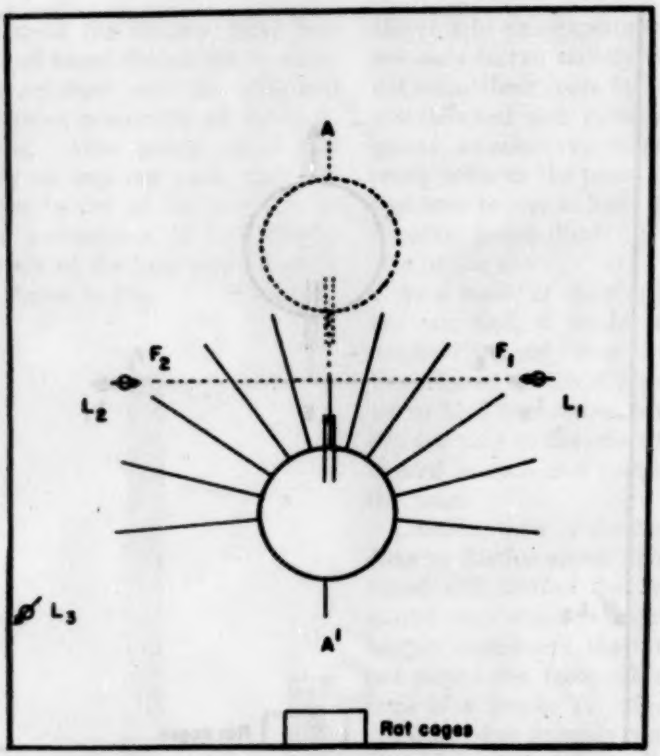


FIG. 19

(From B. F. Ritchie. Ph.D. Thesis: 'Spatial learning in rats.' On deposit in the Library of the University of California, Berkeley, California.)

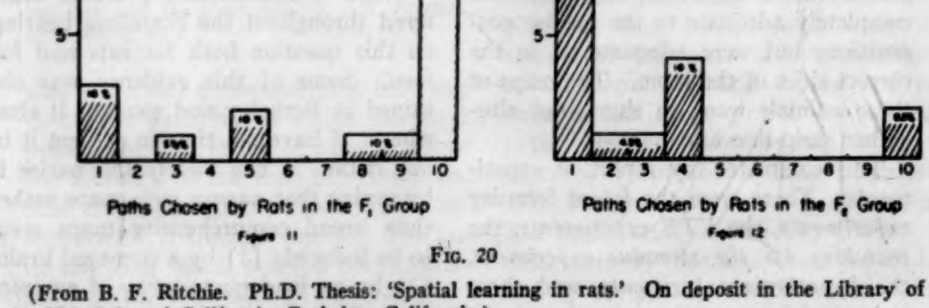


FIG. 20

(From B. F. Ritchie. Ph.D. Thesis: 'Spatial learning in rats.' On deposit in the Library of the University of California, Berkeley, California.)

overdose of repetitions on the original trained-on path and (4) by the presence of too strongly motivational or of too strongly frustrating conditions.

It is this fourth factor which I wish to elaborate upon briefly in my concluding remarks. For it is going to be my contention that some, at least, of the so-called 'psychological mechanisms' which the clinical psychologists and the other students of personality have uncovered as the devils underlying many of our individual and social maladjustments can be interpreted as narrowings of our cognitive maps due to too strong motivations or to too intense frustration.

My argument will be brief, cavalier, and dogmatic. For I am not myself a clinician or a social psychologist. What I am going to say must be considered, therefore, simply as in the nature of a *rat* psychologist's *rat*iocinations offered free.

By way of illustration, let me suggest that at least the three dynamisms called, respectively, "regression," "fixation," and "displacement of aggression onto outgroups" are expressions of cognitive maps which are too narrow and which get built up in us as a result of too violent motivation or of too intense frustration.

(a) Consider *regression*. This is the term used for those cases in which an individual, in the face of too difficult a problem, returns to earlier more childish ways of behaving. Thus, to take an example, the overprotected middle-aged woman (reported a couple of years ago in *Time Magazine*) who, after losing her husband, regressed (much to the distress of her growing daughters) into dressing in too youthful a fashion and into competing for their beaux and then finally into behaving like a child requiring continuous care, would be an illustration of regression. I would not wish you to put too much confidence in the reportorial accuracy of *Time*, but such an extreme case is not too different from many actually to be found in our mental hospitals or even sometimes in ourselves. In all such instances my argument would be (1) that such regression results from too strong a present emotional situation and (2) that it consists in going back to too narrow an earlier map, itself due to too much frustration or motivation in early childhood. *Time*'s middle-aged woman was presented by too frustrating an emotional situation at her husband's death and she regressed, I would wager, to too narrow adolescent and childhood maps since these latter had been originally excessively impressed because of overstressful experiences at the time she was growing up.

(b) Consider *fixation*. Regression and fixation tend to go hand in hand. For another way of stating the fact of the undue persistence of early maps is to say that they were fixated. This has even been demonstrated in rats. If rats are too strongly motivated in their original learning, they find it very difficult to relearn when the original path is no longer correct. Also after they have relearned, if they are given an electric shock they, like *Time*'s woman, tend to regress back again to choosing the earlier path.

(c) Finally, consider the *"displacement of aggressions onto outgroups."* Adherence to one's own group is an ever-present tendency among primates. It is found in chimpanzees and monkeys as strongly as in men. We primates operate in groups. And each individual in such a group tends to identify with his whole group in the sense that the group's goals become his goals, the group's life and immortality, his life and immortality. Furthermore, each individual soon learns that, when as an individual he is frustrated, he must not take out his aggressions on

the other members of his own group. He learns instead to displace his aggressions onto outgroups. Such a displacement of aggression I would claim is also a narrowing of the cognitive map. The individual comes no longer to distinguish the true locus of the cause of his frustration. The poor Southern whites, who take it out on the Negroes, are displacing their aggressions from the landlords, the southern economic system, the northern capitalists, or wherever the true cause of their frustration may lie, onto a mere convenient outgroup. The physicists on the Faculty who criticize the humanities, or we psychologists who criticize all the other departments, or the University as a whole which criticizes the Secondary School system or, vice versa, the Secondary School system which criticizes the University—or, on a still larger and far more dangerous scene—we Americans who criticize the Russians and the Russians who criticize us, are also engaging, at least in part, in nothing more than such irrational displacements of our aggressions onto outgroups.

I do not mean to imply that there may not be some true interferences by the one group with the goals of the other and hence that the aggressions of the members of the one group against the members of the other are necessarily *wholly* and *merely* displaced aggressions. But I do assert that often and in large part they are such mere displacements.

Over and over again men are blinded by too violent motivations and too intense frustrations into blind and unintelligent and in the end desperately dangerous hates of outsiders. And the expression of these their displaced hates ranges all the way from discrimination against minorities to world conflagrations.

What in the name of Heaven and Psychology can we do about it? My only answer is to preach again the virtues of reason—of, that is, broad cognitive maps. And to suggest that the child-trainers and the world-planners of the future can only, if at all, bring about the presence of the required rationality (*i.e.*, comprehensive maps) if they see to it that nobody's children are too over-motivated or too frustrated. Only then can these children learn to look before and after, learn to see that there are often round-about and safer paths to their quite proper goals—learn, that is, to realize that the well-beings of White and of Negro, of Catholic and of Protestant, of Christian and of Jew, of American and of Russian (and even of males and females) are mutually interdependent.

We dare not let ourselves or others become so over-emotional, so hungry, so ill-clad, so over-motivated that only narrow strip-maps will be developed. All of us in Europe as well as in America, in the Orient as well as in the Occident, must be made calm enough and well-fed enough to be able to develop truly comprehensive maps, or, as Freud would have put it, to be able to learn to live according to the Reality Principle rather than according to the too narrow and too immediate Pleasure Principle.

We must, in short, subject our children and ourselves (as the kindly experimenter would his rats) to the optimal conditions of moderate motivation and of an absence of unnecessary frustrations, whenever we put them and ourselves before that great God-given maze which is our human world. I cannot predict whether or not we will be able, or be allowed, to do this; but I *can* say that, only insofar as we *are* able and *are* allowed, have we cause for hope.

THE ROLE OF REWARD IN CONDITIONING THEORY[1]

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When Watson (17), in 1915, gave conditioning a dominant position in his systematic formulation, he initiated a trend in theoretical psychology that still persists. Today, many theoretical systems which attempt to explain relatively complex behavior have, as their bases, principles derived from the data of conditioned response experiments. Because of the generalized implications of this field of investigation, it becomes particularly important to scrutinize carefully and persistently the theories of conditioning.

Hilgard and Marquis (6) have made a methodological analysis of conditioned response experiments which points out a distinction between two types of conditioning *procedure, viz.,* classical and instrumental. The main difference between the two types is that in the former the occurrence of the conditioned response exerts no influence upon the presentation of the unconditioned stimulus (reinforcement), while in instrumental conditioning the achievement of the reward is dependent upon the organism making the appropriate response. The latter is often referred to as selective or trial-and-error learning.

Theorists have disagreed as to whether or not the procedural distinction between kinds of conditioning should be mirrored in their attempts to suggest the basic mechanisms by which learning takes place in the two situations. Guthrie (3) and Hull (8) have insisted that both types of learning are explainable by the same principle, or set of principles, while others (12, 13) have been equally adamant in their assertions that this methodological distinction reflects *two* distinct kinds of conditioning. One of the most provocative and influential presentations of a dual theory is the one reported in this journal several years ago by Maier and Schneirla (12). It is this formulation which will be dealt with in the present paper.

Maier and Schneirla have challenged the tendency "to break down the theoretical distinction between the classical notion of conditioning and that of selective or 'trial-and-error' learning" (12, p. 117), and have emphasized the necessity of distinguishing between these learning situations, not merely in terms of procedure, but also in terms of the basic mechanisms of learning which must be postulated. There are, these writers maintain, two qualitatively different forms of learning. They write:

". . . in the *first stage* of the conditioning *procedure* a neutral stimulus develops excitatory value for a response it did not control. The essential condition for this change in the animal is contiguity between the experience of the neutral stimulus and the experience of the unconditioned stimulus. Then a *second stage* may develop. Once the neutral stimulus has become a

[1] The initial draft of this paper was an outgrowth of a seminar in learning theory at the State University of Iowa. The completion of the paper was delayed by the military service of the authors. Appreciation is extended to Professor Kenneth W. Spence for his valuable criticism of the initial draft. The responsibility for the paper in its present form is, however, entirely ours.

conditioned stimulus, the response it elicits may be rewarded or punished. Unconditioned stimuli such as food and shock can function in this stage in the same way that 'reward' and 'punishment' function in problem-box learning. Thus during this stage a selective learning is involved; as a result the conditioning procedure ceases to be unique and assumes the psychological characteristics of trial-and-error learning. Then, as in all selective learning, the critical response becomes more and more specific the more effectively it leads to reward or to escape from punishment" (12, p. 131).

From this quotation it would appear that in the classical conditioning set-up sheer contiguity of the conditioned and unconditioned stimuli is the necessary condition for establishing the initial associative connection, with reinforcement by reward (or punishment) playing no role. Once the response anticipates the unconditioned stimulus, however, selective learning by reinforcement takes place which serves to strengthen and perhaps modify the response.

Contrasted to this interpretation of conditioning is the one suggested by Hull (8). According to this theory, learning, whether it be the formation of new associations or the differential strengthening of already existent associations, is a ***continuous process of reinforcement***. Reinforcement can be arrived at in two ways:

(1) When a stimulus-response conjunction is closely followed in time by a diminution of a drive, there will result an increment in the tendency for the stimulus to evoke that response.

(2) When a stimulus-response conjunction is closely followed in time with a ***stimulus situation*** which has been closely and consistently associated with a need diminution, there will result an increment to the tendency for that stimulus to evoke that response on subsequent occasions.

The first of these 'laws' is the law of primary reinforcement; the second is the law of secondary reinforcement. It should be noted that a reinforcing state, whether it be primary or secondary, is in some way dependent on need reduction.[2]

The major difference between the Maier-Schneirla theory and the Hull theory is clear. For Hull, learning takes place in the classical conditioning procedure because of reinforcement; for Maier and Schneirla, reinforcement does not become effective until the conditioned response antedates the unconditioned stimulus. All increases in the strength of the associative connection which take place before this antedating are due entirely to contiguity of the conditioned and unconditioned stimulus; reinforcement plays no role.[3] Our analysis of the experimental situations which led Maier and Schneirla to postulate the contiguity stage has in turn led us to believe that such a postulation is not necessary and that Hull's theory of reinforcement is quite adequate to account for the data. The remainder of this paper is an elaboration of this point of view.

We would take immediate exception to the Maier-Schneirla statement that since the unconditioned ***"stimulus initi-***

[2] It should be understood that these statements are a considerable condensation of the complete presentation of Hull's theory of reinforcement. A more complete presentation can be found in several sources (7, 8, 15). Recently Spence (16) has suggested a modification of the theory.

[3] It should be noted that in conditioning responses in which the latency of the response to the conditioned stimulus is longer than the time interval between the conditioned and unconditioned stimuli, no selective learning of the Maier-Schneirla type could take place since the conditioned response would never antedate the unconditioned stimulus.

ates the response it cannot be a reward or punishment for that response" (12, p. 120, italics ours). Here is an obvious confusion. In spite of the fact that a stimulus precedes a response, it is nevertheless quite possible for some component or aspect of that stimulus to succeed the response and thereby reward it. In the case of a conditioned avoidance response to an electric shock on the tissue of the paw, the occurrence of the pain causes the withdrawal response which in turn produces a cessation of the pain. According to Hull's law of primary reinforcement, this constitutes a reinforcing state of affairs, since the response immediately stops the pain instigated by the shock. Although the pain stimulus initiates the response, its *cessation* is a reward for the response and this reward occurs *after* the response.

In the case of the Pavlovian set-up the unconditioned stimulus (food powder) elicits the salivary response, but the need reduction occurs after the response. It seems to us that Hull's analysis of this Pavlovian situation makes it abundantly clear that we can accept reinforcement in classical conditioning in the same manner in which Maier and Schneirla accept it for selective learning. Actually in the classical conditioning situation, probably both primary and secondary reinforcement are operating. Since the sight of food, the smell, the taste, and the kinesthetic excitations of chewing and swallowing food have all been followed by subsequent need reduction, they may act as secondary reinforcing agents. Regardless of whether this reinforcement is primary or secondary or both, the classical conditioning situation seems to us to represent a fairly clear-cut exhibition of a situation in which a stimulus which elicits a response can also reinforce that response. The rejection of this principle seems unwarranted and it appears that it is this rejection by Maier and Schneirla which forces their postulation of the contiguity type of learning.

Maier and Schneirla offer the results of an experiment by Brogden, Culler and Lipman (1) as support for their two-stage theory. These investigators trained guinea pigs to run in a rotating cage in response to a tone as the conditioned stimulus and shock as the unconditioned stimulus. For one group (avoidance group) the occurrence of the conditioned running response led to the avoidance of the shock. For a second group (non-avoidance group), the shock always occurred regardless of what response the animals made. The experimenters measured only the frequency of the running response to the sound of the tone. As regards this response, the learning curves for the two groups were quite similar for approximately the first 100 trials. From that point the curves diverge, with the avoidance group showing a greater frequency of running to the tone than the non-avoidance group. Maier and Schneirla have concluded that these results on the first 100 trials were evidence of a common associative stage of learning for both groups, *i.e.*, contiguity of the conditioned and unconditioned stimuli was the sufficient condition for the associative formation. After 100 trials selective learning is believed to account for the different behavior of the two groups.

There are other plausible interpretations which may be given the Brogden, Culler, and Lipman results. First we may call attention to the results of an experiment by Girden (4). Girden found that the development of the precise form of the conditioned response is not entirely a function of the conditioning process, but results in part from the repeated application of the unconditioned stimulus alone. According to such a view the similarity of results on

the two groups of guinea pigs was not necessarily entirely a function of an associative stage but rather a result of the presentation of the unconditioned stimulus without reference to the conditioned stimulus. In Girden's experiment the unconditioned stimulus was administered to the foreleg of the dog without the conditioned stimulus being applied. The number of applications of the unconditioned stimulus varied with different animals. Girden reports that "the type of conditioned response procured depended upon the degree of preliminary training with the unconditioned stimulus. If the unconditioned response was still at the reflexive stage, then the conditioned response assumed the same form. If, however, sufficient preliminary training with the unconditioned stimulus had been first given, then the voluntary conditioned response appeared in short order" (4, p. 679).

The difference in the interpretation of the Brogden, Culler, and Lipman experiment suggested by Girden's results and the interpretation given by Maier and Schneirla could be put to test by eliminating the conditioned stimulus for the first 75 to 100 trials, leaving *all* other experimental conditions constant. These 75 to 100 trials of the unconditioned stimulus alone could not be considered as part of the associative stage according to the Maier-Schneirla view, and therefore the difference in the conditioned running response of the two groups should be delayed for approximately 75 to 100 *additional* trials during which the conditioned and unconditioned stimuli were *combined*. If the similarity of the curves of the two groups for the initial 100 trials (as found by Brogden, Culler, and Lipman) is due to some extent to the adjustment to the unconditioned stimulus, we would then expect the curves to diverge appreciably prior to the 200th trial. We would expect that following a series of 100 'adjustment trials' to the unconditioned stimulus, the curves for the two groups would separate rather quickly after a few combined presentations of the conditioned and unconditioned stimuli.

An experiment which would bear upon the relative adequacy of the Maier-Schneirla formulation as contrasted to the one supported by the present writers would have the following single variation on the experimental procedure used by Brogden, Culler, and Lipman. If after the 100th trial the two groups were reversed, *i.e.*, the avoidance group was made the non-avoidance group and vice versa, Maier and Schneirla would be forced to predict results similar to those obtained in the original experiment. This would be expected since, according to the Maier-Schneirla postulation, the two groups learn the same thing during the first 100 trials, namely, an association between the experiences of the conditioned and unconditioned stimuli. Differences arise in the two groups only when selective learning becomes prominent, which in this case is supposed not to occur until after 100 trials. The prediction made by a continuous reinforcement theory following such a suggested variation would be that the superiority of the new avoidance group would be appreciably delayed when contrasted to the original findings of Brogden, Culler, and Lipman. It is this prediction which brings into sharp experimental focus the difference between the two theories.

Experiments by Culler and Mettler (2), Girden, Mettler, Finch, and Culler (5), and Shurrager and Culler (14), have demonstrated that conditioned responses can be established in the decorticate and spinal dog. Since conditioned responses formed in these operated dogs were of a generalized, diffuse nature, Maier and Schneirla argue that such conditioning is illustrative

of their pure contiguity-type learning. They assume that for selective learning to occur (resulting in a specific, well-localized response), the presence of the cortex is essential. Again we find Maier and Schneirla providing a rather restricted interpretation of reinforcement theories. These empirical findings with respect to decorticate and spinal animals are in no way contradictory to a continuous reinforcement theory of conditioning since there is nothing physiologically specific to an operational definition of a reinforcing state of affairs. The cortex may account for the precision of the response which usually develops in the intact animal, but it does not seem necessary to assume that the cortex is essential for a theory of reinforcement as promulgated here. In this connection attention should again be called to the results of the Girden experiment (4) referred to above. Girden has found that the ability to make specific responses is not due to the conditioning exclusively but also to the repeated presentation of the unconditioned stimulus (shock) alone. It appears that it is this degree of organization of behavior to the unconditioned stimulus which the decorticate or spinal animal is incapable of developing. Therefore the differences in the degree of precision of conditioned responses of normal and decorticate animals may not be a function of different stages of learning but merely a function of the animal's inability to react in a certain manner to a specific stimulus because of lack of neural structure necessary for the evocation of a precise motor response.

Maier and Schneirla cite the results of an experiment by Loucks (10) as further substantiation of the two-phase theory. Loucks used a direct faradic shock to the motor area of the cortex to elicit leg flexion in a dog. Using a buzzer for a conditioned stimulus, conditioning failed to occur. When, however, food was introduced after each flexion response produced by cortical shock, conditioning readily took place. According to Maier and Schneirla conditioning failed to take place in the first instance because "the unconditioned stimulus must have effective sensory relations as well as a purely motor function" (12, p. 125). Presumably the shock to the cortex does not provide these effective sensory relations and hence is not a 'true' unconditioned stimulus. This, according to Maier and Schneirla, prevents the first or contiguity stage of conditioning from taking place. Addition of the reward immediately brings selective learning (second stage or kind) into play without having been preceded by the first stage.

Our interpretation of Loucks' data is obvious. In the first instance there is no need diminution and hence the essential condition for learning was not present. Addition of the food provided this essential condition. Loucks interprets the data in essentially the same manner. The results, then, are in no way contradictory to a continuous reinforcement theory.

An important question is raised in the assertion by Maier and Schneirla that once food is introduced in Loucks' experimental situation it becomes a selective learning problem. It would seem, however, that the operations followed are identical to those demanded by Maier and Schneirla for the contiguity stage of learning, *i.e.*, ". . . in the *first stage* of the conditioning *procedure* a neutral stimulus develops excitatory value for a response it previously did not control" (12, p. 131). In the Loucks experiment the buzzer (neutral stimulus) acquired excitatory value to elicit leg flexion (a response it previously did not control). To state that this is a case of selective learning seems to be a convenient though somewhat in-

consistent attempt at explanation as if to avoid granting reinforcement as a fundamental principle of learning during the initial or first stage. It should be noted that if the Maier-Schneirla interpretation ("foreleg flexion became associated with reward just as string-pulling becomes associated with reward in the problem-box situation" [p. 126]) of the Loucks experiment is carried over into another more familiar situation inconsistencies in their interpretation become clearer. For example, if one group of rats were trained in the Skinner box in the usual manner and another trained to press to bar *only* when a buzzer sounded, Maier and Schneirla would interpret both cases as selective learning even though a new association (buzzer-bar depression) was formed in the second group.

In further support of their thesis, Maier and Schneirla refer to a study by Light and Gantt (9) which showed that it is possible to establish a conditioned leg withdrawal without the response taking place during training. The motor nerve which normally effects withdrawal was crushed so that during the training period the response could not take place. After the nerve was regenerated it was shown that the response took place upon presentation of the conditioned stimulus. Maier and Schneirla write:

"This result could not have occurred had not the training somehow established an association between the buzzer and shock experience. It thus follows that making the critical response is not essential for the development of true conditioning" (12, p. 126).

It is presumed that by "true conditioning" Maier and Schneirla mean association by contiguity—the first stage. A reinforcement theory does not demand, of course, that the response *per se* has to occur for reinforcement to occur. Since the cessation of the shock produces a reinforcing state of affairs, a functional relation can be established between the conditioned stimulus and the potential overt response (efferent discharge). Light and Gantt point out that it is reasonable to assume that the efferent discharge for foot withdrawal was made regardless of the fact that it did not produce the response while the nerve was crushed.

Considering the Light and Gantt study in conjunction with Loucks' results, Maier and Schneirla conclude that making the response is necessary for selective learning but not necessary for conditioning. We know of no data which bear directly on this problem but it appears that the experiment proposed by Maier and Schneirla would provide pertinent evidence toward the differences in the interpretation given the Loucks experiment. If in the second phase of Loucks' experiment the motor nerves to the leg were crushed, Maier and Schneirla predict that negative results (no learning) would occur, since they interpret this situation as being selective learning. We would predict quite the opposite since no assumptions are made by a reinforcement theory as concerns the necessity of the response actually occurring before learning will take place.

Additional evidence (not cited by Maier and Schneirla) relating to the point of 'effective sensory relations' is given by the study of Loucks and Gantt (11). These experimenters were able to establish a conditioned leg withdrawal when the unconditioned stimulus was a faradic shock applied directly to the dorsal spinal roots but *not* when the response was produced by applying the shock to the spinal cord and thus avoiding the excitation of pain pathways. These writers state:

"An unconditioned stimulus which leads to a reflex movement without exciting nociceptive neurones, or other of similar category,

seemingly constitutes an inadequate basis for establishing a conditioned avoiding movement" (11, p. 426).

It thus appears that an unconditioned stimulus resulting in a simple sensory (non-painful) discharge is not enough to establish conditioning when it is preceded by neural impulses arising from a conditioned stimulus. When need reduction is included, however, a new association is formed.

CONCLUSIONS

It has been our intention to evaluate the Maier-Schneirla dual-mechanism theory of conditioning solely from the point of view of whether or not the data demand the postulation of such a two-stage theory. Consequently we have neither considered those experimental studies (cited by Maier and Schneirla) which emphasize the selective learning phase, nor have we concerned our discussion with the nature of the physiological basis constituting association formation. The former point is ignored because there is no real difference in the interpretation of these studies by a two-stage theory as contrasted with a continuous reinforcement theory. The discussion of the physiological basis of learning is overlooked because a non-reductive theory such as Hull's (discounting his *language* trends) is not dependent upon physiological evidence indicating any type of cortical mechanism.

Our conclusion is that Maier and Schneirla have not made a convincing case for the necessity of a two-stage theory of learning. A single principle of reinforcement will handle all the data which they have cited as requiring the postulation of two kinds of learning.

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CHARACTERISTICS OF DISPERSIONS BASED ON THE POOLED MOMENTARY REACTION POTENTIALS ($_{S}\dot{\bar{E}}_{R}$) OF A GROUP[1]

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INTRODUCTION

In an earlier publication (3) we have pointed out that the soundness of a type of measurement in science is indicated jointly by (a) its fertility in yielding quantitative scientific laws and (b) the quantitative consistency of the various emerging laws and their constants among themselves and with other relevant phenomena (1). In any really systematic science, even such as the physics of Isaac Newton, consistency phenomena exist in profusion. A commonplace example of consistency in natural laws is seen in the operation of the primary law of falling bodies in the secondary or derived law of the ordinary pendulum. Unfortunately, in the field of behavior the number of such interlocking quantitative laws is very small and the idea that behavioral laws should show this type of consistency is practically nonexistent. Indeed, such a validity requirement would doubtless be rejected by certain psychologists.

The earlier publication referred to above presented evidence that the power of the Thurstone psychophysical quantification technique is fertile in yielding the data from which such laws may be formulated. The objective of the present study will be, in the main, to present evidence of the consistency of this quantificational methodology and its deliverances with relevant known facts and principles.

There is generally recognized to be some uncertainty regarding the scientific status of psychological scaling methods with which the present quantificational procedure is closely allied. This is due in part to their heavy dependence upon the *a priori* assumption of the statistical 'normality' of the distribution of the differences involved, which is at the base of all psychophysical procedures. Such a dependence is demonstrated by the universal use of tables of the normal probability integral in this type of quantification. Moreover, in most scaling situations, as Thurstone long ago pointed out, there is the complication that the responses of many subjects are pooled; this may easily generate a distribution different from that of an individual subject, which alone is involved in ordinary psychophysics. This has led us to seek among other things a means to

[1] This is the fourth study in the series by the present authors. The experimental work, as described in the first study of the series (2), was performed by Mr. Felsinger, the statistical analysis was carried out by Mr. Yamaguchi and Mr. Gladstone, and Mr. Hull supervised the study as a whole and prepared the manuscript in frequent conference with the others. We are indebted to Mr. Charles B. Woodbury for the careful drawing of the several graphs which appear in the text, and to Mr. Hardy C. Wilcoxon and especially to Miss Ruth Hays for some final computations. It should be noted that the appropriateness of the words "pooled" and "of a group" in this title is reduced by the evidence reported in footnote 4, which was added just prior to publication.

test the truth of the normal probability assumption in the present experimental situation, and so the soundness of the quantification performed.

The empirical data involved in the quantification here under consideration consisted of the pooled learning latency scores of 59 albino rats. The act learned was very simple, merely the lateral movement of a straight brass tubular manipulandum which projected five-eighths inch into the animal's chamber in a sound-shielded box. One trial per day was given for a minimum of 60 successive days. In most cases the number of days was considerably more (2). The methodology employed in the quantification was an adaptation of Thurstone's psychophysical method of paired comparisons, Case III (9, 10, 11), as described in detail elsewhere (7). In general the method used in securing evidence regarding the consistency of the outcome of our quantification was based upon the fact that its application to our data yielded the empirical equation,

$$_SE_R = 2.845\ _St_R^{-.483} - .599. \quad (1)$$

Here $_St_R$ is the median reaction latency of our 59 animals at a given stage of training and $_SE_R$ is the reaction potential of the same animals taken as a group, as determined by our method of quantification.

This brings us to the concept of momentary reaction latency ($_S\dot{t}_R$) as distinguished from the median reaction latency ($_St_R$). Now, by definition,

$$\text{the median } _S\dot{t}_R = {}_St_R. \quad (2)$$

Moreover, there is excellent reason to believe that reaction potential ($_SE_R$) is subject to momentary oscillation ($_SO_R$) or variability, exactly as reaction latency is empirically known to be (6, pp. 304 ff.), which would naturally produce momentary reaction potential ($_S\dot{\bar{E}}_R$). And since $_SE_R$ at a given stage of learning is determined from the reactions of the group as a whole, it must represent a central tendency of the $_S\dot{\bar{E}}_R$'s involved, which is closely equivalent to and possibly identical with the median $_S\dot{\bar{E}}_R$. As a first approximation we accordingly write the equation,

$$\text{the median } _S\dot{\bar{E}}_R = {}_SE_R, \quad (3)$$

which parallels equation (2) in terms of reaction potential.

On these assumptions it is evident that the first members of equations (2) and (3) may be substituted for the second members in equation (1). This being the case it is tempting to assume, at least as a first approximation, that not only the medians but all the other corresponding momentary values of each equation could also be substituted in equation (1). This assumption yields the following equation,

$$_S\dot{\bar{E}}_R = 2.845\ _S\dot{t}_R^{-.483} - .599. \quad (4)$$

It is clear from the context that half of the latency responses at the first trial in our present learning experiment must fall below the lowest median (trial 1) involved in the derivation of equation (1), which determined the arbitrary zero value of $_SE_R$ as there expressed. It follows from an inspection of that equation that these and all momentary latencies which are longer than the longest median latency will yield negative $_S\dot{\bar{E}}_R$'s. But these negative momentary reaction potentials, though small, are just as real as any; the negative sign indicates merely that they fall below the lowest median $_S\dot{\bar{E}}_R$, *i.e.*, below that of the first successful trial of the group as a whole. This rela-

tionship, however, is of no particular significance for our present purposes. Moreover, it is likely to be confusing. We accordingly eliminate it simply by dropping the $-.599$ from the right-hand member, leaving the working equation,

$$ {}_S\dot{\bar{E}}_R = 2.845\ {}_S\dot{i}_R{}^{-.483}, \qquad (5)$$

which in effect places the zero of the scale of ${}_S\dot{\bar{E}}_R$'s .599 of a unit lower than does equation (1). It is evident, of course, that the soundness of equations (4) and (5) will (and must) be subject to the same quantitative consistency tests regarding soundness as are the more empirical aspects of the quantification.

With equation (5) available we should be able to (1) substitute in it any of the empirically given momentary reaction latencies, (2) calculate the corresponding momentary reaction potentials, and (3) with a large number of these available and suitably pooled and plotted determine with some directness the shape and range of the ${}_S\dot{\bar{E}}_R$ dispersion as derived from the behavior of pooled subjects. From these same values it should be possible by subtraction to (4) find the *differences* between momentary reaction potentials, (5) pool these differences into an appropriate distribution, (6) determine the shape and range of this difference distribution and thus determine in an objective manner the soundness of our quantificational procedure, including the assumptions underlying the writing of equations (4) and (5) above.

CHARACTERISTICS OF THE DISPERSION OF POOLED ${}_S\dot{\bar{E}}_R$ VALUES AT THE LATENCY ASYMPTOTE

Our first task will be to examine the shape and range of the distribution of momentary reaction potential values at the latency asymptote of our data, *i.e.*, at the limit of learning. It happened that each of the 59 animals employed in the present investigation gave 10 reaction latencies at its latency asymptote and 10 more on the following 10 trials. These 20 asymptotic latencies of each animal were all converted into ${}_S\dot{\bar{E}}_R$'s by means of a table constructed with the aid of equation (5), yielding 20 rows of values, one for each asymptotic trial, with 59 values per row, one for each subject. An inspection of these data indicated that there was considerable oscillatory variation in these ${}_S\dot{\bar{E}}_R$ values, quite as would be expected from the momentary oscillation principle (6, p. 313). In addition, the first ten trials upon the whole showed a slight tendency to have larger ${}_S\dot{\bar{E}}_R$ values than did the second ten as a group, which presumably was caused by a slight tendency for the ${}_S\dot{i}_R$'s to increase with continued training beyond the asymptote. Now, 20 rows of 59 values each comprise a total of 1180 momentary reaction potentials, enough to yield a fair indication of the function sought.

Unfortunately these data involve two independent sources of variability, (1) the oscillation function proper which appears in the responses of each organism considered by itself, and (2) the variability in the behavior of the several organisms from one another when the oscillation function is eliminated, *e.g.*, by taking the central tendency of the ${}_S\dot{\bar{E}}_R$'s for each individual subject. Proceeding to the isolation of the oscillation factor involved in the dispersion of the 1180 ${}_S\bar{E}_R$'s, we first found the mean of the 20 asymptotic momentary reaction potential values yielded by each of the 59 animals. These means are obviously to a considerable extent free

from the $_sO_R$ variability, the variability remaining being due in the main at least to the individual differences of the separate organisms. We next proceeded to eliminate so far as feasible the factor of individual differences just considered by adding to or subtracting from each set of 20 yielded by each subject an amount sufficient to make the means of the 59 groups the same. The distribution of the 1180 $_s\dot{\bar{E}}_R$'s so corrected is shown in Fig. 1.

To casual inspection this dispersion bears a striking resemblance to the normal or Gaussian distribution. It has a central maximum from which it tapers off with approximate symmetry at each side, at first rapidly and then more slowly until the slope is almost zero. But more exact tests of normality than mere inspection are required in the present situation. For this purpose Karl Pearson has devised two rather precise quantitative functions, β_1 and β_2 (8, 14). The indication of symmetry is given by β_1, a perfectly normal distribution yielding a value of .00. On the other hand, β_2 gives an indication of the extent to which the distribution tends to bunch or arch in the middle, one which is strictly normal yielding a value of 3.00. If the value of $\beta_2 > 3.00$ the distribution bunches in the middle more than is normal, and is called leptokurtic; if, on the other hand, the value of $\beta_2 < 3.00$ the dispersion tendency to bunch or arch in the middle is less than normal and is

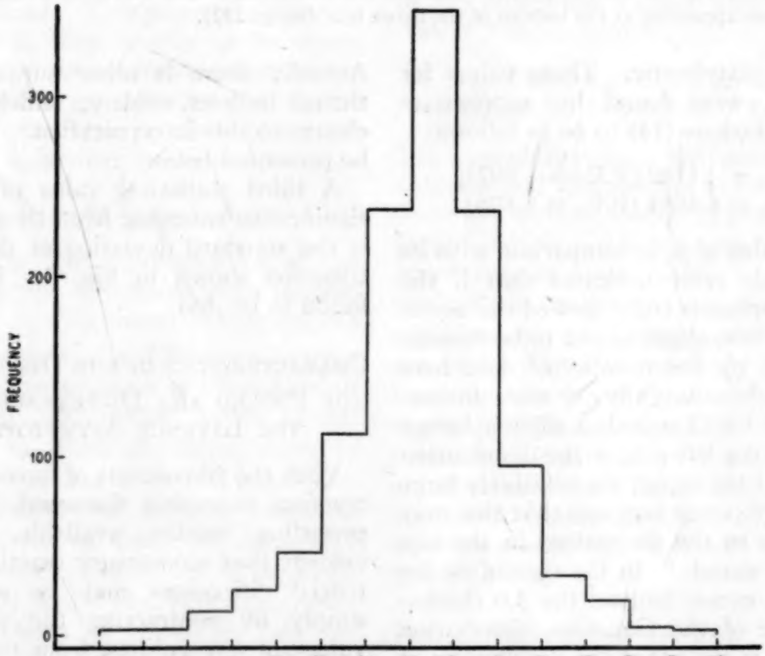

FIG. 1. Graphic representation of the distribution of 1180 calculated $_s\dot{\bar{E}}_R$ values after approximate correction for the distorting effects of the individual differences. The unit of measurement of the values appearing at the bottom of the figure is σ' (see p. 222).

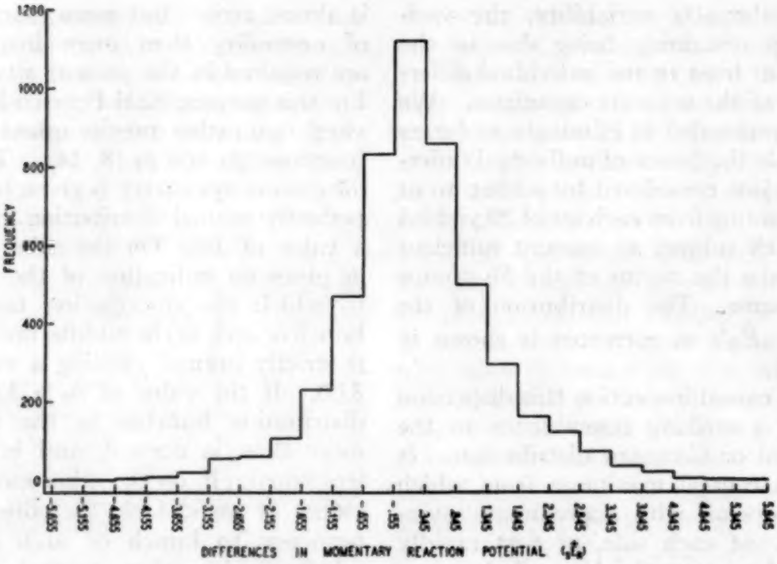

Fig. 2. Distribution of 5000 calculated momentary reaction-potential ($_S\dot{E}_R$) differences of 59 albino rats at the limit of latency reduction due to training. The unit of measurement of the values appearing at the bottom of the figure is σ' (see p. 222).

called platykurtic. These values for Fig. 1 were found by appropriate computations (14) to be as follows:

$$\beta_1 = \ .1162 \text{ (P.E.} = \ .097),$$
$$\beta_2 = 4.4664 \text{ (P.E.} = 1.696).$$

The value of β_1 in comparison with its probable error indicates that if the distribution is truly skewed the asymmetry is so slight as not to be demonstrated by the number of data here available. Actually, a close inspection of Fig. 1 reveals a slightly longer tail at the left side of the distribution than at the right; the relatively large probable error suggests that this may be due to the limitations in the size of the sample. In the case of β_2 the 1.4664 excess beyond the 3.0 characteristic of the Gaussian distribution makes it clear that the distribution of Fig. 1 is leptokurtic, though the size of the probable error indicates that the present data are not sufficient to establish this as a fact in the absence of other supporting evidence. Actually there is other supporting, though indirect, evidence which leads clearly to this interpretation. It will be presented below.

A third statistical value of some significance emerging from these data is the standard deviation of the distribution shown in Fig. 1. It was found to be .861.

Characteristics of the Dispersion of Pooled $_S\dot{E}_R$ Differences at the Latency Asymptote

With the 20 columns of momentary reaction potentials discussed in the preceding section available, it is evident that momentary reaction potential differences may be secured simply by subtracting the parallel values of any column from those in any other column. By this procedure the $_S\dot{E}_R$ differences secured concern only individual organisms. When these 20 columns containing 59 $_S\dot{E}_R$'s each in pairs are employed in various

combinations it is evident that very large numbers of such differences may be secured.

Of the many thousands of differences thus available a systematic selection of 5000 was taken as follows. Beginning at the first asymptotic trial the $_S\dot{\bar{E}}_R$ values were subtracted from those of the corresponding row of the second column, those of the second column from those of the corresponding row of the third, those of the third from those of the fourth, and so on until 500 differences were secured, the positive and negative differences being tabulated separately. Next, the $_S\dot{\bar{E}}_R$'s of the first column were subtracted from those of the third, those of the second from those of the fourth, and so on (skipping one column) until 500 more differences were found. This procedure was continued in such a way as to secure more and more remote comparisons, the number of columns (*i.e.*, trials) skipped in obtaining the various sets of 500 differences being as follows:

0, 1, 2, 3, 4, 5, 6, 7, 8, and 10.

The graphic representation of this total set of differences may be seen in Fig. 2. To casual inspection this dispersion of $_S\dot{\bar{E}}_R$ differences bears a striking resemblance to the normal or Gaussian distribution, much as did that of the 1180 $_S\dot{\bar{E}}_R$ values shown in Fig. 1. It has a central maximum and it tapers off with approximate symmetry at each side, at first rapidly and then more slowly until the slope is almost zero. But here again more precise tests of normality are required. The β values for Fig. 2 were found by appropriate computations (**14, 8**) to be as follows:

$$\beta_1 = .0234 \text{ (P.E.} = .03),$$
$$\beta_2 = 3.9873 \text{ (P.E.} = .73).$$

The value of β_1, in comparison with its probable error, indicates that if the distribution is skewed the asymmetry is so slight as not to be demonstrated by this considerable number of data. In the case of β_2, the statistical indication is that the .9873 deviation beyond 3.00 has a ratio of 1.352 to its probable error, which corresponds to a probability of about 1 in 5 that it is due to the chance of sampling.

There is, however, other evidence bearing on these points. The β_1's and β_2's were calculated separately for each set of 500 differences which entered into the 5000. About half of the β_1's were in one direction and half in the other, which largely disposes of the skew evidence. On the other hand, every one of the ten sets of 500 differences showed a β_2 in excess of 3.00, as follows:

5.493, 4.203, 4.733, 4.256, 4.355,
4.263, 4.929, 4.464, 4.612, 4.553.

The probability that ten successive values would all come out in the same direction without special causation is $(1/2)^{10}$, which yields a value of only one chance in about 1024. We conclude with confidence that the distribution of $_S\dot{\bar{E}}_R$ differences represented by Fig. 2 is genuinely leptokurtic in shape and by analogy that the $_S\dot{\bar{E}}_R$ values represented in Fig. 1, from which Figure 2 was derived, also are distributed in a leptokurtic manner. Still further evidence supporting this conclusion is given below (pp. 230, 235).

THE CONSTRUCTION OF TWO TYPES OF EMPIRICAL LEPTOKURTIC FREQUENCY TABLE

In addition to the formal evidence that our $_S\dot{\bar{E}}_R$ difference distribution is leptokurtic in form, we desired a

somewhat detailed comparison of it with that of the Gaussian probability integral as represented in tables of that function. As a first step in securing such a comparison the standard deviation of the distribution of differences represented by Fig. 2 was calculated and found to be 1.2288. Next, a table was made of the frequency of the empirically obtained differences in ${}_S\dot{\bar{E}}_R$'s with an interval of .1 unit, the unit in question being the standard deviation of the oscillation function as employed in the quantificational method used by us (7). We shall represent this unit by the symbol σ'. An inspection of Fig. 2 will show that each interval there represented includes five such .1 σ' frequency intervals. Since the present distribution was found to be practically symmetrical, the two wings or halves of this distribution were combined, which doubled the available population for our table with the division falling at the midpoint of the modal interval. This involved the division of the middle .1 σ' of the modal set of five (Fig. 2) leaving half of the middle .1 σ' interval at the beginning of the series as such tables are ordinarily constructed. In order to make a uniform interval of a full .1 σ' throughout the series, this half interval was combined with half of the next whole .1 σ' interval for the first interval of our table, the other half of the first whole .1 σ' interval with half of the next whole .1 σ' interval, and so on throughout the entire series of 5000 differences. Incidentally this resulted in the smoothing out of a few slight irregularities in the progressive decrease in the combined frequencies (doubtless caused largely by dropped decimals) as the σ' distance increased from the middle of the original distribution.

The next step was to determine the x/σ values for our table. As already pointed out, the σ of our difference distribution was found to be 1.2288 in terms of the present σ' unit, *i.e.*,

$$\sigma = 1.2288\,\sigma'.$$

Accordingly,

$$\sigma' = \frac{\sigma}{1.2288} = .8138\,\sigma.$$

Dividing through by 10, we have

$$\frac{\sigma'}{10} = .1\,\sigma' = .08138\,\sigma,$$

which means that each .1 σ' interval used in our original distribution corresponds to .08138 σ.

Now, a convenient method of expressing the ordinate of an empirical distribution such as the present one is to state the number from a population of 10,000 that falls within a range of .01 σ at any given distance from the mean. In the present distribution, 468.5 of the population of 10,000 (*i.e.*, 2 × 5000 produced by pooling the two wings of our population of differences) are found in the first interval of .08138 σ at one side of the mean. This is 8.138 times the .01 σ range customarily employed. Accordingly, 468.5 ÷ 8.138 = 57.59, which is the approximate ordinate value at the middle of the interval, *i.e.*, at .08138 ÷ 2 = .04069 σ; or, in round numbers, at .041 σ at one side of the mean. Proceeding in this manner the next observed frequency of our data, 453.5, yields an ordinate value of 55.73, which falls in the middle of the next interval or .04069 σ + .08138 σ = .122 σ at one side of the mean. By procedures exactly analogous to those used in these two examples we secured the remaining paired entries appearing as the first two columns in Table I. By way of comparison there has been placed in

TABLE I

Ordinate values of a leptokurtic distribution with a β_2 value of 3.9873 based on a population equivalent to 10,000, with parallel values of the normal probability integral. (Taken from Guilford, 4, p. 531.)

σ distance from mean	Ordinate population for .01 σ interval on base of 10,000		σ distance from mean	Ordinate population for .01 σ interval on base of 10,000	
	leptokurtic distribution	normal distribution		leptokurtic distribution	normal distribution
.041	57.59	39.86	2.157	3.62	3.87
.122	55.73	39.61	2.238	3.69	3.25
.203	47.92	39.10	2.319	3.56	2.70
.285	44.11	38.25	2.401	3.44	2.24
.366	42.76	37.25	2.482	3.32	1.84
.448	41.90	36.05	2.563	2.83	1.51
.528	38.09	34.67	2.645	1.90	1.19
.610	33.18	33.12	2.726	1.60	.96
.692	27.46	31.44	2.808	1.41	.77
.773	24.08	29.66	2.889	1.35	.61
.854	22.30	27.80	2.970	1.23	.48
.935	20.40	25.65	3.052	1.35	.38
1.017	17.57	23.71	3.133	1.23	.30
1.099	14.75	21.79	3.215	.68	.22
1.180	12.96	19.89	3.296	.43	.17
1.261	11.43	18.04	3.377	.43	.13
1.343	11.55	16.26	3.459	.43	.10
1.424	10.14	14.56	3.540	.31	.08
1.505	7.93	12.95	3.621	.31	.06
1.587	6.76	11.27	3.702	.31	.04
1.668	5.78	9.89	3.784	.12	.031
1.750	5.71	8.65	3.865	.26	.022
1.831	5.59	7.48	3.947	.26	.016
1.912	5.22	6.44	4.028	.12	.012
1.994	4.92	5.51	4.109	.12	.009
2.075	4.06	4.59	4.191	.31	.006

a parallel third column of each set of three the corresponding frequency yielded by the normal probability integral, also as based on a population of 10,000.

An examination of these two sets of ordinate values shows that adjacent to the center or mean of the distribution the leptokurtic ordinate is much greater than the normal, *e.g.*, 57.59 versus 39.86 at the first interval. However, as the σ distance increases the leptokurtic values decrease very rapidly until at $x/\sigma = .61$ the two ordinates are practically identical. For larger values of x/σ the leptokurtic ordinates grow progressively less than the normal, then increase again, relatively, until again at $x/\sigma = 2.157$ the two ordinates have reversed their position after which the leptokurtic ordinate values remain the larger to $x/\sigma = 4.19$. This of course is generally characteristic of leptokurtic distributions (14, p. 158).

The differences between leptokurtic data and those of the normal probability integral as presented in the preceding section seemed to call for a special table constructed as are the normal probability tables now in general use. For one thing, a quantification of some leptokurtic data may be desired and we know of no appro-

priate table with which to do it. Accordingly, from the original difference distribution from which Table I was derived we also constructed Table II. This table is essentially a transcription of our empirical results. While it gives the main substance necessary for a table analogous to

TABLE II

Table of the empirical values of a leptokurtic probability distribution whose β_2 is 3.9873. It shows the fraction of the area lying between zero and x/σ, together with the corresponding value of x/σ itself.

Area from $x/\sigma=0$	x/σ	Area from $x/\sigma=0$	x/σ
.047	.0814	.465	1.9531
.092	.1628	.469	2.0345
.131	.2441	.472	2.1159
.167	.3255	.475	2.1973
.202	.4069	.478	2.2786
.236	.4883	.481	2.3600
.267	.5697	.484	2.4414
.294	.6510	.486	2.5228
.316	.7324	.489	2.6042
.336	.8138	.490	2.6855
.354	.8952	.491	2.7669
.371	.9766	.493	2.8483
.385	1.0579	.494	2.9297
.397	1.1393	.495	3.0111
.408	1.2207	.4957	3.0924
.417	1.3021	.4968	3.1738
.426	1.3835	.4973	3.2552
.434	1.4648	.4977	3.3366
.441	1.5462	.4980	3.4180
.446	1.6276	.4984	3.4993
.451	1.7090	.4986	3.5807
.456	1.7904	.4988	3.6621
.460	1.8717	.4991	3.7435

tables of the probability integral used in psychophysical scaling, the steps are far too coarse for convenient use, especially those at the beginning, and the area intervals do not have the stepwise regularity customary in such tables. Accordingly Table III was derived from Table II by linear interpolations so that the area columns increase by equal steps of .005.

Figure 3 gives a graphic representation of the empirical leptokurtic probability function as presented in Tables II and III, and for purposes of comparison a parallel graph of the corresponding normal probability function. It is evident from an inspection of these two tables and the graph that a rather large and systematic difference in the quantificational results must arise when an inappropriate table is applied to leptokurtic paired-comparison data in securing quantificational results, quite as already demonstrated. The next-to-last columns of Table II show that the absolute difference between the two increases as one moves away from the zero point of comparison, exactly as appears on the early portions of the graph. Even so, the *relative* size of the differences grows less from the very onset of the two curves.

THE DISTORTION OF SCALE-SEPARATION VALUES BY THE USE OF AN INAPPROPRIATE FREQUENCY TABLE

It is clear from an inspection of Tables I, II, and III together with Fig. 3 that in case a scale quantification is attempted by means of the paired-comparisons procedure as ordinarily employed, a distortion of the results secured will occur if the frequency table does not match the distribution characteristics of the data serving as the basis of the quantification. This is because the ordinate frequencies of a leptokurtic distribution which appear early in the table, *i.e.*, which occupy positions close to the middle of the distribution, are so much greater per σ unit from the mean than is the case in a Gaussian distribution. Thus the first ordinate entry of Table I (57.59) is over 44 per cent greater than the Gaussian equivalent (39.86) at the same distance from the mean (.041 σ). This brings it about that when a per cent

TABLE III

Leptokurtic distribution table derived from Table II by means of linear interpolations in such a way that the 'area' increases by steps of .005 of the total statistical population.

Area from $x/\sigma=0$	x/σ	Area from $x/\sigma=0$	x/σ	Area from $x/\sigma=0$	x/σ	Area from $x/\sigma=0$	x/σ
.005	.009	.130	.242	.255	.538	.380	1.029
.010	.017	.135	.253	.260	.551	.385	1.058
.015	.026	.140	.264	.265	.564	.390	1.092
.020	.035	.145	.276	.270	.579	.395	1.126
.025	.043	.150	.288	.275	.594	.400	1.162
.030	.052	.155	.298	.280	.609	.405	1.199
.035	.061	.160	.310	.285	.624	.410	1.239
.040	.069	.165	.321	.290	.639	.415	1.284
.045	.078	.170	.333	.295	.655	.420	1.329
.050	.087	.175	.344	.300	.673	.425	1.375
.055	.096	.180	.356	.305	.692	.430	1.424
.060	.105	.185	.367	.310	.710	.435	1.476
.065	.114	.190	.379	.315	.729	.440	1.535
.070	.123	.195	.391	.320	.749	.445	1.611
.075	.132	.200	.402	.325	.769	.450	1.693
.080	.141	.205	.414	.330	.789	.455	1.774
.085	.150	.210	.426	.335	.810	.460	1.872
.090	.159	.215	.438	.340	.832	.465	1.953
.095	.169	.220	.450	.345	.855	.470	2.062
.100	.180	.225	.462	.350	.877	.475	2.197
.105	.190	.230	.474	.355	.900	.480	2.333
.110	.200	.235	.486	.360	.924	.485	2.482
.115	.211	.240	.499	.365	.948	.490	2.686
.120	.221	.245	.512	.370	.972	.495	3.011
.125	.232	.250	.525	.375	1.000	.500	

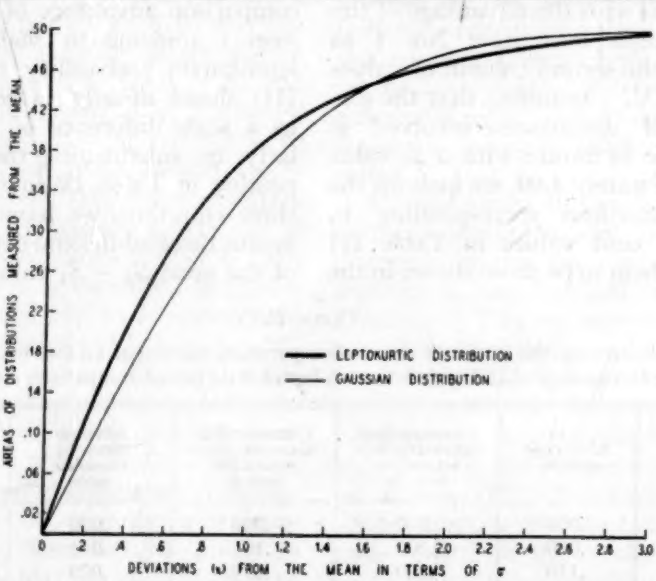

FIG. 3. **Graphic representation of the empirical leptokurtic probability function given in Tables II and III and, for comparison, the corresponding values of a table of the normal probability integral.**

difference in a leptokurtic comparison is not very great the σ scale value yielded by a Gaussian table may be as much as 44 per cent too large.

As suggested, this distortion is greatest where the per cent difference resulting from the comparison is small and grows progressively less as the per cent difference grows larger. A comparison of Tables II and III with a table of the normal probability integral, *e.g.*, Guilford's (**4**, p. 537 ff.), shows that the amount of this excess continues to decrease up to where the per cent (area) difference reaches the neighborhood of .44, after which the scale values are too small. This general relationship is shown graphically by Fig. 3.

It happens that when this sort of distortion exists its presence is revealed by a characteristic inconsistency which appears in a certain type of table utilized in the quantification technique invented by Thurstone (**4**, p. 228). Let it be supposed that we have a series of paired-comparison proportions with the advantage of the various items over item No. 1 as shown in the second column of values in Table IV. Assuming that the dispersion of differences involved is leptokurtic in nature with a β_2 value of approximately 4.00, we look up the scale separations corresponding to these per cent values in Table III and find them to be those shown in the third column of Table IV. Turning to the corresponding Gaussian scale separations in Guilford's table (**4**, p. 537 ff.), we find that they are as appear in the fourth column of Table IV. The values in columns 3 and 4 are distinctly different from one another. The differences between the two sets of scale-separation values are shown in the fifth column of Table IV and the per cent that the Gaussian values are of the leptokurtic values appears in the last column. It will be noticed that the absolute differences *increase* as one goes down the column, whereas the per cent differences *decrease*.

In addition to the direct method of determining the amount of a given scale separation as shown in Table IV, there are several indirect methods; and if the table used is accurate and a very large number of data are involved, each of the several determinations will agree exactly with the others. Take, for example, the scale separation of items 1 and 2: the paired-comparison advantage of item 2 over item 1 amounts to .060, which our leptokurtic probability table (Table III) shows *directly* as corresponding to a scale difference of .105. Similarly, by substituting the values appearing in Table IV in the following three equations we have by indirect means three additional determinations of the same $S_2 - S_1$ value but, it will

TABLE IV

Table illustrating the origin of the scale-separation distortion arising from the use of a table of the Gaussian probability function on leptokurtic paired-comparison data.

No. of item	Advantage over S_1	Corresponding leptokurtic scale separation from S_1	Corresponding Gaussian scale separation from S_1	Absolute excess by Gaussian table	Per cent excess by Gaussian table
S_1	.000	.000	.000	.000	
S_2	.060	.105	.151	.046	44
S_3	.110	.200	.279	.079	39
S_4	.155	.298	.399	.101	34
S_5	.200	.402	.524	.122	30

be noted, ***derived largely from different parts of the probability table from the portion used in the direct determination:***

By direct determination, $S_2-S_1=.105$. Also,

$$S_2-S_1=(S_3-S_1)-(S_3-S_2)$$
$$=(.200-.000)-(.200-.105)$$
$$=.200-.095=.105$$

and

$$S_2-S_1=(S_4-S_1)-(S_4-S_2)$$
$$=(.298-.000)-(.298-.105)$$
$$=.298-.193=.105$$

and

$$S_2-S_1=(S_5-S_1)-(S_5-S_2)$$
$$=(.402-.000)-(.402-.105)$$
$$=.402-.297=.105$$

In the case of the use of the normal probability table on the same leptokurtic paired-comparison data as those just considered, the outcome is quite different. We have already seen that the *direct determinations* of $S_2 - S_1$ by the normal probability table is definitely larger than .105, namely, .151. Secondly, we need to find the equivalent of a direct determination of the values of $S_3 - S_2$, $S_4 - S_2$, and $S_5 - S_2$. In column 3 of Table IV we see that $S_3 - S_2$ amounts to $.200 - .105$, or $.095\,\sigma$, which corresponds (Table III) approximately to a paired-comparison advantage of .055; $S_4 - S_2$ amounts to $.298 - .105$, or $.193\,\sigma$, which corresponds (Table III) approximately to a paired-comparison advantage of .105; $S_5 - S_2$ amounts to $.402 - .105$, or $.297\,\sigma$, which corresponds (Table III) approximately to a paired-comparison advantage of .155; and these three per cent paired-comparison advantages correspond respectively to Gaussian scale differences (as given in Guilford's table, **4**, p. 537 ff.) of .138, .266, and .399. Substituting these Gaussian values in addition to those given in the fourth column of Table IV in the three following equations, we have:

By direct determination, $S_2-S_1=.151$

$$S_2-S_1=(S_3-S_1)-(S_3-S_2)$$
$$=.279-.138=.141$$

$$S_2-S_1=(S_4-S_1)-(S_4-S_2)$$
$$=.399-.266=.133$$

$$S_2-S_1=(S_5-S_1)-(S_5-S_2)$$
$$=.524-.399=.125$$

Thus we see that where the frequency table matches the data distribution form, the multiple determination of scale separations agrees exactly with the direct determination. On the other hand, where leptokurtic data are treated with a normal probability table (a) the value of a given scale separation as directly determined is considerably greater than are the values of those indirectly determined, ***the latter decreasing in size the more remote they are from the point of direct determination.*** This means that in general (b) ***the longer the series of indirectly determined values, the greater this relative difference will be;*** it also means that in general (c) ***the directly determined value will be greater than the mean of the indirectly determined values.***

A convenient illustration and exemplary verification of the tabular distortion of the three types of scale separation just deduced appears as Table V. This is reproduced with some modifications and extensions from Guilford's well-known text (**4**, pp. 227, 228), the data having been taken from a classical empirical study published by Thurstone some twenty years ago (**11**). Naturally with a limited number of data available this table (based on the results from 239 subjects) will not display the regularity characteristic of the largely theoretical deduction just given which

TABLE V

A table illustrating the multiple estimates of the scale separation of each item of a series from the next (4, p. 228). The directly determined (d.d.) scale separations are set in bold-faced type; the remaining values are secured by indirect determination (i.d.).

Numbers of items used in original quantification	The scale-separation estimates							
	1 from 2	2 from 3	3 from 4	4 from 5	5 from 6	6 from 7	7 from 8	8 from 9
1	*	.617	−.041	.716	−.107	.635	.000	.243
2	.589	*	.472	.595	−.021	.469	.528	*
3	.840	.366	*	.483	.726	.735	.000	.000
4	.327	.556	.282	*	−.004	.541	.184	.380
5	.448	.668	−.077	.842	*	.655	.397	.306
6	.362	−.079	.653	.511	.327	*	.255	.701
7	.528	−.345	.847	.397	.597	.385	*	.384
8	.000	.183	.663	.184	.739	.228	.412	*
9	*	*	.283	.258	.344	.545	.134	.662
Means excluding the value in bold-faced type (i.d.)	.417	.267	.400	.449	.325	.544	.209	.336
Direct (d.d.) less indirect determinations (i.d.)	.172	.099	−.118	.393	.002	−.159	.203	.326
Ratio of $\frac{\text{d.d.}}{\text{i.d.}}$	1.412	1.370	.705	1.875	1.006	.708	1.971	1.970

* These spaces have no entries because no significant values are yielded by the original paired-comparison data.

tacitly assumes an infinite number of data. A glance shows that each of the values of the diagonal row set in bold-faced type (the directly determined scale separations) is upon the whole greater than the remaining values of its respective column. Moreover, the farther one goes down each column below the directly determined value, the smaller upon the whole become the ostensible scale separations. For example, if we take the first four columns of the table and calculate the means of the scale separations of the five diagonal rows, beginning with the directly determined row, we have:

.520, .457, .511, .350, and .235.

Thus one of our theoretical deductions (a) finds examplary verification and a lawful consistency between principles and fact is illustrated.

Similarly, the extreme of the i.d. series of values is further from the d.d. value in the same series on the first and last two columns (Table V) than on the two middle columns. Therefore the ratios of $\frac{\text{d.d.}}{\text{i.d.}}$ should upon the whole be larger for the four end columns than for the four middle columns. A glance at the ratios appearing at the bottom of Table V shows that this clearly tends to be the case, the means being 1.07 and 1.68 respectively, the former only 64 per cent of the latter. Thus a second theoretical deduction (b) finds exemplary verification.

Finally, the means of the indirectly determined scale separations (at the bottom of Table V) may be seen to be upon the whole less than are the

results of the direct determinations (in bold-faced type), six of the eight pairs of values showing an excess in favor of the directly determined scale separations. Thus a third theoretical deduction (c) finds exemplary verification and another lawful consistency is illustrated.[2]

As our main evidence on the matter of the scale-separation distortion produced by the application of a Gaussian probability table to a leptokurtic distribution of paired-comparison data, we have the quantificational results of the present investigation which have been described in considerable detail elsewhere (2, 7). In this investigation 25 points on a learning curve (see the upper part of Fig. 4) were quantified by the use of a table of the normal probability integral. The 24 scale-difference values resulting from the direct comparisons of the 25 $_S\dot{\bar{E}}_R$ items involved (and analogous to the values shown in bold-faced type in Table V) appear in the second column of Table VI (d.d.). The third column gives the parallel means of the remainder of the same columns consisting of numerous indirect determinations or estimates (i.d.) of exactly the same item separations. These values are analogous to the means at the third row from the bottom of Table V. The fourth column shows the ostensible scale-separation distortion of the several scaled items, analogous to the differences appearing in the next-to-last row of Table V.

[2] It must be noted at this point that Table V here utilized as an expository aid represents only part of the data from Thurstone's investigation. It will be shown below (Tables VII and VIII) that the mean value of the directly determined series of the entire experiment is 56 per cent greater than that of the indirectly determined series, and that the difference in question has a t statistic of 2.50 with 18 degrees of freedom, which proves it to be significant at the 2 per cent level.

Now, owing mainly to the large number of subjects used by Thurstone, very few cases of negative values appear in the columns of the directly and indirectly determined scale separations of Table V, whereas such values occur quite occasionally in Table VI. Because of the novelty of this type of table and the relationship involved we shall explain our method of calculating the distortion effects. In the case of Table V the directly determined scale-separation of items 2 and 3 is .366, while the mean of the corresponding indirectly determined scale separation is .267. The distortion obviously is $.366-.267=.099$. Similarly in the case of items 3 and 4 the directly determined value is .282 and the mean of the indirectly determined values is .400. The difference here also is $.282 - .400 = -.118$, *i.e.*, the difference is truly negative. But suppose that both of the $2 - 3$ values were negative? In that case both would be on the negative side of the probability distribution and the $-.366$ would indicate the same sort of scaling distortion as would obtain on the other side of a probability distribution and of exactly the same amount. However, we can *not* simply subtract,

$$-.366 - (-.267) = -.099,$$

because this would give a negative .099 which would mean the *opposite* of a distortion. Consequently we must change both signs before we subtract, thus:

$$+.366 - (+.267) = \quad .099$$

and

$$+.282 - (+.400) = -.118.$$

Also, when the sums of the columns of direct and indirect determinations are found the signs must be ignored, though this is not the case with the

TABLE VI

Columns 2, 3, and 4 of this table present the evidence concerning the scale-separation distortion due to the use of a normal probability table on leptokurtic paired-comparison data of the present study. Columns 5, 6, and 7 show parallel results from the application of an empirical leptokurtic table to the same (leptokurtic) paired-comparison data.

Trials involved in scale separation determinations	Results secured by applying a Gaussian frequency table to leptokurtic paired comparison data			Results secured by applying an empirical leptokurtic frequency table to leptokurtic paired-comparison data		
	Directly determined scale separations (d.d.)	Indirectly determined scale separation means (i.d.)	Direct determinations minus indirectly determined means	Directly determined scale separations (d.d.)	Indirectly determined scale separation means (i.d.)	Direct determinations minus indirectly determined means
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1–3	.377	.320	.059	.162	.321	−.159
3–5	.700	.477	.223	.428	.379	.049
5–7	.592	.327	.265	.426	.341	.085
7–9	.483	.398	.085	.400	.291	.109
9–11	.547	.290	.257	.544	.297	.247
11–13	−.108	−.164	−.056	−.099	−.173	−.074
13–15	.157	.238	−.081	.143	.256	−.113
15–17	.428	.260	.168	.387	.260	.127
17–19	.219	.189	.030	.176	.167	.009
19–21	.025	.017	.008	.018	−.005	.023
21–23	−.204*	.211*	**	−.144	−.155	−.011
23–25	.333	.315	.018	.247	.271	−.022
25–27	.549	.210	.339	.419	.181	.238
27–29	−.156	−.334	−.178	−.106	−.328	−.222
29–31	.059	.161	−.102	.048	.210	−.162
31–34	.394	.395	−.001	.312	.342	−.030
34–37	−.384	−.347	.037	−.289	−.322	−.033
37–40	.096	.014	.082	.067	−.007	.074
40–43	.544	.533	.011	.379	.452	−.073
43–46	−.333	−.377	−.044	−.215	−.301	−.086
46–49	.301	.345	−.044	.173	.212	−.039
49–52	−.110	.020	.130	−.063*	.071*	**
52–55	.108	.046	.062	.068	.092	−.024
55–58	.135	.075	.060	.082*	−.051*	**
Sums	7.140	5.852	1.328	5.250	5.363	−.087
Means	.3104	.2544	.0577	.2386	.2438	−.00395

*** Data involving cases where the direct and the indirect determinations have different signs and the latter is more than .04. In such cases the scale-separation distortion is regarded as indeterminate and is left blank with the symbol **.**

fourth column which shows the distortions. By the same logic, where the indirect determination is zero the sign of the parallel direct determination is changed in case it is negative, which also gives a positive distortion result. In practice, where the indirect determination is of opposite sign yet lies within .04 of zero the differences are utilized. But otherwise where the paired values are of opposite sign the distortion effect is considered indeterminate and such values are disregarded in all computations. They are placed in the table merely in the interest of record completeness.

The third column of Table VI and

the first row of Table VIII show that the mean distortion of our own data is positive, *i.e.*, in the direction to be expected, to the amount of .0577 with a t statistic of 2.003. The distortion effect amounts to 22.7 per cent of the mean of the i.d. scale separation values. Since this determination has 21 degrees of freedom the difference has significance at the 6 per cent level.

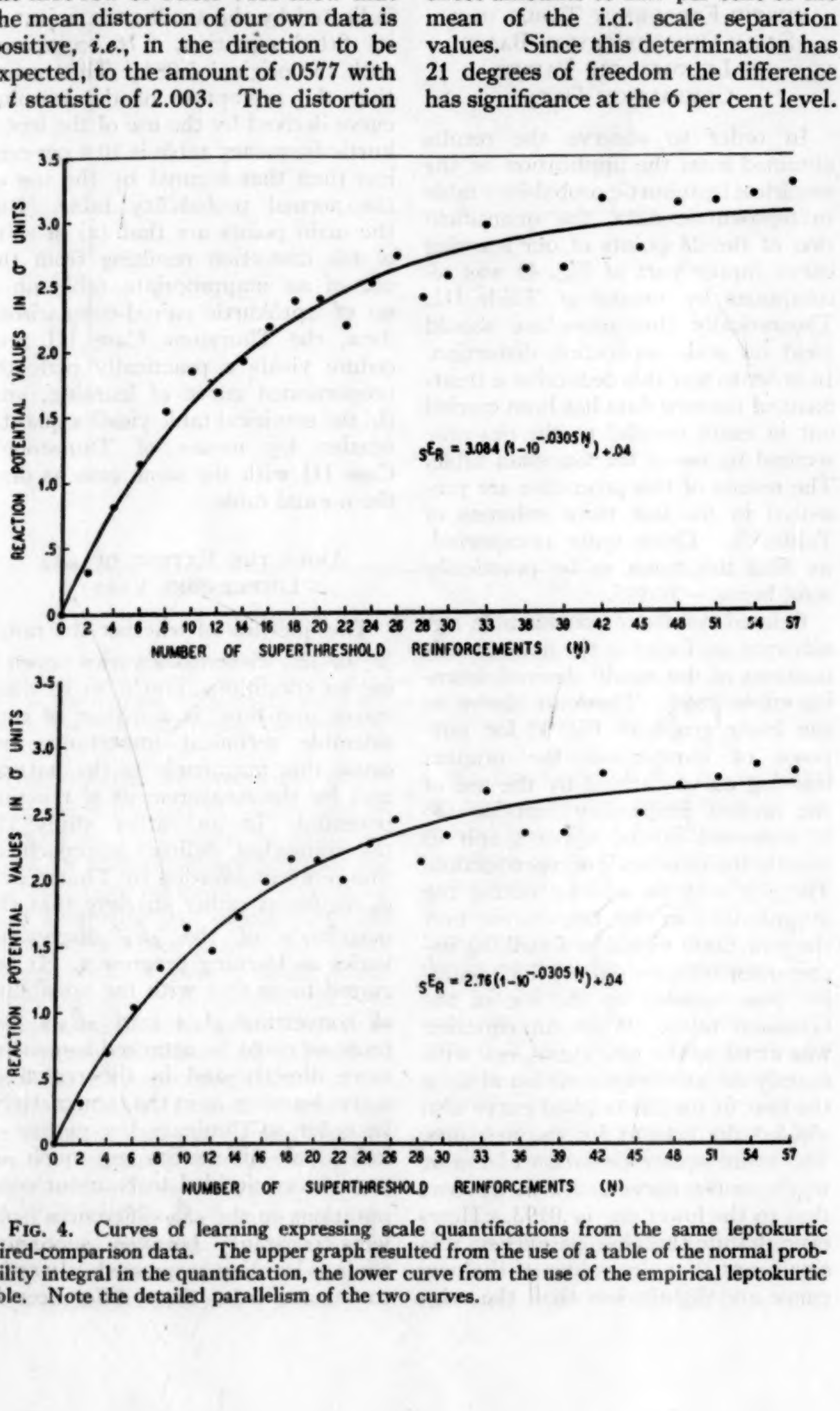

FIG. 4. Curves of learning expressing scale quantifications from the same leptokurtic paired-comparison data. The upper graph resulted from the use of a table of the normal probability integral in the quantification, the lower curve from the use of the empirical leptokurtic table. Note the detailed parallelism of the two curves.

THE USE OF THE EMPIRICAL LEPTOKURTIC FREQUENCY TABLE IN A SCALE QUANTIFICATION BASED ON LEPTOKURTIC PAIRED-COMPARISON DATA

In order to observe the results obtained from the application of the empirical leptokurtic probability table to leptokurtic data, the quantification of the 25 points of our learning curve (upper part of Fig. 4) was recomputed by means of Table III. Theoretically this procedure should yield no scale separation distortion. In order to test this deduction a treatment of the new data has been carried out in exact parallel to the outcome secured by use of the Gaussian table. The results of this procedure are presented in the last three columns of Table VI. There, quite as expected, we find the mean to be practically zero, being $-.00395$.

Related results of considerable significance are found in the final quantifications of the newly derived learning curve itself. These are shown as the lower graph of Fig. 4; for purposes of comparison the original learning curve derived by the use of the normal probability function (**3**) is presented in the upper graph to exactly the same scale of reproduction. There it may be seen by noting the irregularities in the two curves that the new table yields to a striking degree quantification values which parallel those secured by the use of the Gaussian table. When an equation was fitted to the new curve, one with exactly the same exponent found to be the best fit for the original curve also yielded the best fit for the new one. The mean square deviation of the fit to the upper curve is .0275, whereas that to the lower one is .0193. However, despite this close parallelism it is significant that the values of the new curve are slightly less than the original values, as is characteristically indicated by the smaller coefficient of its fitted equation, 2.76, compared with the original 3.084. This means that the asymptote of the learning curve derived by the use of the leptokurtic frequency table is 10.6 per cent less than that secured by the use of the normal probability table. But the main points are that (a) in spite of the distortion resulting from the use of an inappropriate table on a set of leptokurtic paired-comparisons data, the Thurstone Case III procedure yields a practically perfectly proportioned curve of learning, and (b) the empirical table yields a quantification by means of Thurstone's Case III with the same ease as does the normal table.

DOES THE EXTENT OF $_S\dot{\bar{E}}_R$ DISPERSIONS VARY?

The question of whether the range (σ) of $_S\dot{\bar{E}}_R$ dispersions varies depending on conditions, and if so to what extent and how, is a matter of considerable technical importance because this magnitude is the natural unit for the measurement of reaction potential. In an earlier study (**7**) the somewhat indirect approach to this problem afforded by Thurstone's σ_k suggested rather strongly that the magnitude of the $_S\dot{\bar{E}}_R$ dispersions varies as learning progresses. It occurred to us that with the possibility of converting $_S\dot{I}_R$'s into $_S\dot{\bar{E}}_R$'s this problem could be attacked somewhat more directly and in the region of active learning, as in the former study. In order to eliminate the matter of individual differences as much as possible we decided to base our computations on the $_S\dot{\bar{E}}_R$ differences (subjects constant) between successive trials as learning progressed. In order to remove from these differences so

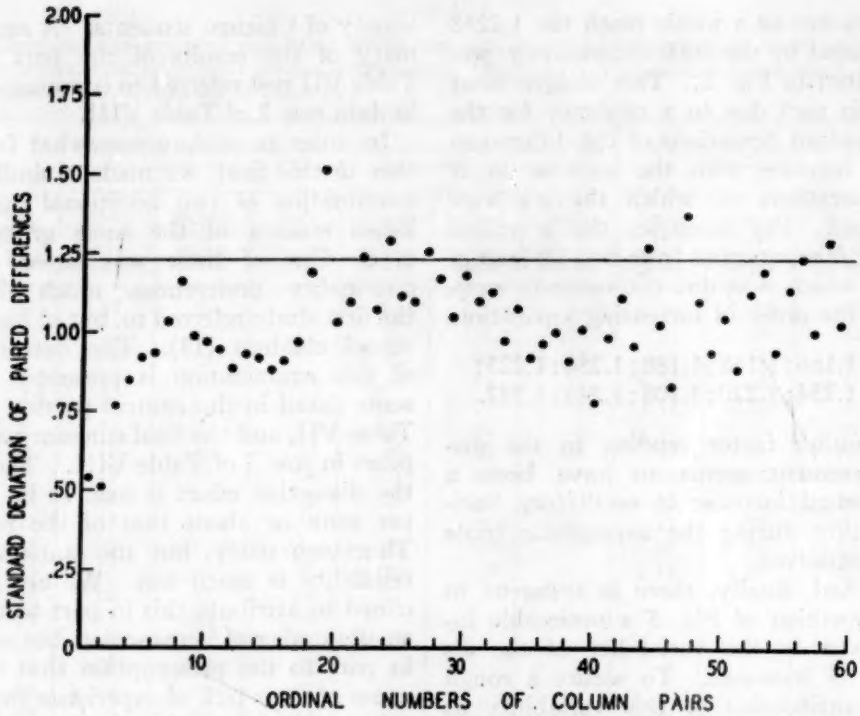

FIG. 5. Graphic representation of the standard deviations of the $_S\dot{\bar{E}}_R$ differences of 59 subjects found between the successive responses at the first 60 reinforced trials of the learning of a simple response. The light horizontal line just below 1.25 represents the standard deviation of the 5000 differences of these subjects at the limit of learning (Fig. 2).

far as possible the excess due to the progressive increase in $_S\dot{\bar{E}}_R$ caused by learning as N increases, we found the mean for each column of 59 items and the difference between the means for each successive pair of columns; this amount was added to each item in the column with the smaller mean, which made the means of each of the two compared columns equal. The values of one column were then subtracted from the corresponding values of the other, the signs of the differences being retained, and the standard deviation of each set of 59 differences was calculated. Thus there was one less set of differences (and so of σ's) than there were columns of original $_S\dot{\bar{E}}_R$ values. These standard deviations are represented in Fig. 5.

An inspection of this figure shows at once that the magnitude of the several dispersions during the learning process is by no means constant, quite apart from chance variations which, incidentally, are considerable. Thus is a theoretical hypothesis (6, p. 314) definitely disproved by empirical fact. The dispersion magnitudes at first conform to a simple increasing function of N but this is soon followed by a series of undulations with crests at ordinal difference numbers 7–9; 20–24; and 55–59. This function is too complex for even approximate determination by ordinary curve-fitting procedures.

It is also noteworthy that the rise in the present series of σ values, even near the asymptote ($N = 60$),

does not as a whole reach the 1.2288 yielded by the 5000 differences represented in Fig. 2. This disagreement is in part due to a tendency for the standard deviations of the differences to increase with the increase in N separations on which the σ's were based. For example, the σ values for N separations from 0 to 10 (except 9, which was not determined) were, in the order of increasing separation,

1.186; 1.168; 1.188; 1.256; 1.225; 1.254; 1.220; 1.208; 1.249; 1.222.

Another factor tending to the disagreement seems to have been a gradual increase in oscillatory variability during the asymptotic trials themselves.

And, finally, there is apparent to inspection of Fig. 5 a noticeable increase in the variability of the σ's as N increases. To secure a rough quantification of this variability as distinct from presumably genuine progressive changes in σ as a function of N, we placed the data of Fig. 5 on a large-scale drawing, drew with care a smoothed curve among their central tendencies, read off the successive deviations of the individual entries from this line, and averaged these deviations in successive groups of 11 or 12 trials. These means were

.043; .088; .067; .110; and .102.

DISCUSSION

On an earlier page we referred to evidence of scale-separation distortion appearing in one of Thurstone's well-known studies of quantification based on paired comparisons mediated by a table of the normal probability integral (11). We now present the somewhat detailed evidence in the first three data columns of Table VII. This study was based on the nationality preferences, apparently, of University of Chicago students. A summary of the results of the part of Table VII just referred to is contained in data row 2 of Table VIII.

In order to explore somewhat further in this field, we made a similar examination of two additional published studies of the same general type. One of these was based on nationality preferences, much like the first study referred to, but of high-school children (13). The outcome of this examination is presented in some detail in the central portion of Table VII, and the final summary appears in row 3 of Table VIII. There the distortion effect is seen to be 54 per cent, or about that of the first Thurstone study, but the statistical reliability is much less. We are inclined to attribute this in part to the small number of items scaled, but also in part to the presumption that because of their lack of experience these subjects had not developed normal nationality preferences and so depended much more on mere guessing than did the more mature subjects of the other nationality preference study. The latter situation may be indicated by the relatively large number of four negative signs in the first two columns not matched by a parallel negative sign in the associated column.

The third Thurstone study which we examined was based on the judgments of 200 subjects concerning the extent to which a series of wet-dry propositions favored prohibition. The detailed data are shown as the last three columns of Table VII and the statistical outcome is presented in row 4 of Table VIII. Here again we find a per cent distortion of approximately the same size as the effects yielded by the other two Thurstone studies. The level of significance of this study and that of our own study are about the same,

TABLE VII

Summary of evidence indicating scale-separation distortion contained in three paired-comparison quantificational studies published by Thurstone. This table is constructed in a manner exactly analogous to Table IV.

Items under comparison	Nationality preferences by 239 adult human subjects (11)			Nationality preferences by 250 high-school children (13)			Wet-dry judgments by 200 subjects (12)		
	Direct determination values	Indirect determination means	Scale separation distortions	Direct determination values	Indirect determination means	Scale separation distortions	Direct determination values	Indirect determination means	Scale separation distortions
1–2	1.27	.82	.45	.28	.16	.12	1.20	.26	.94
2–3	.59	.52	.07	.11	.31	−.20	.64	.40	.24
3–4	−.08*	.07*	**	−.10	.02	.12	.36	−.02	.38
4–5	.31	.17	.14	.36	.13	.23	−.29	−.18	.11
5–6	.26	.03	.23	.25	.17	.08	2.17	2.11	.06
6–7	.45	.22	.23	.07	−.04	.11	−.16	−.23	−.07
7–8	.72	.50	.22	.39	.30	.09	.10	.02	.08
8–9	.07	.01	.06	.07	.00	.07	.58	.05	.53
9–10	.04	.09	−.05	−.03*	.14*	**	−.10	.01	.11
10–11	.28	.07	.21	.10	.12	−.02	.27	.23	.04
11–12	.03	.14	−.11	.20	.08	.12	−.05	−.08	−.03
12–13	.21	.23	−.02	−.06*	.30*	**	2.17	1.55	.62
13–14	.12	.15	−.03						
14–15	.11	.03	.08						
15–16	.05	.20	−.15						
16–17	.09	.12	−.03						
17–18	.42	.10	.32						
18–19	.13	.02	.11						
19–20	.42	.31	.11						
20–21	.25	.00	.25						
Sums	5.82	3.73	2.09	1.93	1.33	.72	8.09	5.14	3.01
Means	.3063	.1963	.11	.193	.133	.072	.674	.428	.251

each lacking a little of the 5 per cent level sometimes taken as the lower limit of dependability when unsupported by other evidence. Actually, the fact that all four studies summarized in Table VIII agree in the nature of the outcome, most of them with fair levels of significance, constitutes rather strong corroborative evidence of the genuineness of the tend-

TABLE VIII

A summary of the scale-distortion effects found in five paired-comparison quantification studies.

Study		Amount of distortion		Reliability			
Author	Reference	Absolute	Per cent of i.d.	No. of subjects	t	Degrees of freedom	Level of significance
Present	Present	.0577	22.7	59	2.003	21	6 per cent
Thurstone	11	.11	56.0	239	2.50	18	2 per cent
Thurstone	13	.072	54.0	250	1.674	9	14 per cent
Thurstone	12	.251	58.6	200	2.145	11	6 per cent
Guilford	5	.035	10.0	1	.597	5	56 per cent

ency of quantifications based on the paired comparisons of pooled subjects to yield this type of distortion.

We may also speak specifically concerning the meaning of the internal consistency encountered in the present study as being in the scientific tradition and indicating in so far the soundness of the quantification method employed and the general behavior theory which lies behind it. We have five points: (1) the characteristic leptokurtic scale-separation distortion of pooled $_{S}\dot{\bar{E}}_{R}$'s *indirectly* indicating that the presence of a leptokurtic distribution existed *before* the quantification was completed; (2) the *direct* determination of the leptokurtic distribution of pooled $_{S}\dot{\bar{E}}_{R}$'s and $_{S}\dot{\bar{E}}_{R}$ differences was made *after* the signs of the leptokurtic distortion were in existence; (3) since No. 1 and No. 2 were quite separate determinations, an error in either one could not have produced the other; (4) moreover, these two determinations agree in that each one demands the outcome produced by the other in the light of current theories of our quantificational procedure; (5) finally, the use of the empirical leptokurtic probability table in the quantification of the very same paired-comparison data produced a set of values which did *not* show the characteristic scale-separation distortion, as is also demanded by the theory behind our quantificational procedure.[3]

Lastly we must show that the present modified procedure is not only consistent with itself and with other known facts and principles but is fertile in the sense that eight equations constituting basic laws or constants may be derived from our revised and corrected quantificational procedure. The first is

$$_{S}E_{R}=2.76\,(1-10^{-.0305\dot{N}})+.04. \qquad \text{I}$$

Substituting -2 for $\dot{N}$ in the above equation (3, p. 514), we have

$$\begin{aligned} Z &= 2.76\,(1-10^{-.0305\times(-2)})+.04 \\ &= 2.76\,(1-10^{.061})+.04. \\ \therefore\quad Z &= -.377. \qquad \text{II} \end{aligned}$$

Now (3, p. 514), Z falls $.377+.04$ or .417 below the first overt superthreshold reaction. For this reason the coefficient of the new equation as based on absolute zero must be $2.76 + .417$, or 3.177, *i.e.*,

$$_{S}E_{R}=3.177\,(1-10^{-.0305\dot{N}}). \qquad \text{III}$$

Accordingly (3, p. 515),

$$M'=3.177. \qquad \text{IV}$$

In a similar manner (3, p. 516),

$$\begin{aligned} _{S}L_{R} &= 3.177\,(1-10^{-.0305\times1.5}) \\ &= 3.177\,(1-10^{-.0458}) \\ &= 3.177\times.1. \\ \therefore\quad {_{S}L_{R}} &= .3177. \qquad \text{V} \end{aligned}$$

Now, substituting in the following

[3] In case the degree of leptokurtosis varies from one project to another this will present a practical difficulty. However, with modern calculating devices it should not be too laborious to compute several leptokurtic tables covering the range over which data vary according to the equation given by Pearson (8, p. lxiii):

$$y = y_0 \frac{1}{\left(1+\frac{x^2}{a^2}\right)^m}.$$

As a beginning, tables could perhaps be made which are based on β_2's of 3.75, 4.00, and 4.50. Meanwhile we may be able to establish a dependable rule which will enable us to estimate rather closely in advance which table will be required. If not it will be fairly easy to apply our test for scale-separation distortion, or some improved version of it, to the Thurstone Case III procedure and try the suitability of one table after another to the paired-comparison data until one is found which will yield a zero or near zero degree of distortion.

equation we have (3, p. 516)

$$\frac{{}_sL_R}{M'} = \frac{.3177}{3.177} = .1.$$

$$\therefore \quad {}_sL_R = .1\,M'. \qquad \text{VI}$$

Also, since according to current theory the parenthetical part of the equation for reaction potential is the habit portion, we have (3, p. 516)

$$_sH_R = 1 - 10^{-.0305\dot{N}}. \qquad \text{VII}$$

In regard to reaction latency, we have the median $_sE_R$ values from the lower curve of Fig. 4 and the corresponding median reaction latencies in 3, p. 517. Plotting these and fitting an equation we have

$$_sE_R = 2.59\ {}_st_R{}^{-.466} - .59 \qquad \text{VIII}$$

and the mean square deviation amounts to only .0069.

SUMMARY AND CONCLUSIONS

1. Reaction latencies at the limit of learning of a group of 59 albino rats were converted into momentary reaction potentials by means of the empirical equation,

$$_s\dot{\bar{E}}_R = 2.845\ {}_s\dot{t}_R{}^{-.483}.$$

2. The distribution of the $_s\dot{\bar{E}}_R$'s was found directly to display a deviation from the Gaussian curve known as the leptokurtic form of kurtosis.

3. Five thousand differences between the $_s\dot{\bar{E}}_R$ values from the above grouped subjects were found directly to be distributed in substantially the same leptokurtic manner, this close resemblance between two such distributions duplicating that characteristic of a pair of Gaussian distributions similarly related.[4]

4. The standard deviation of this $_s\dot{\bar{E}}_R$ difference distribution was found to be 1.426 times that of the original $_s\dot{\bar{E}}_R$ distribution, a very close approximation to the theoretical ratio for a pair of Gaussian distributions in this relationship, which is $\sqrt{2}$ or 1.414.

5. It was found possible rather easily to construct from our 5000 differences in $_s\dot{\bar{E}}_R$ two types of empirical probability table, one of them analogous to the usual table of the normal probability integral used in scaling.

6. From a study of these two tables, in comparison with the corresponding types of normal probability table, a deduction was made regarding a characteristic scale-separation distortion which would appear in the process of quantifying leptokurtic paired-comparison data by means of the normal table.

7. A search revealed the presence of this distortion in our own data and also in three analogous standard published studies.

8. We found entirely feasible the use of the second form of empirical probability table, with a certain amount of interpolation, upon the original paired-comparison data of the present study for purposes of quantification by Thurstone's Case III method.

9. This yielded a quantification without the characteristic scale-separation distortion associated with the use of the normal probability table.

10. Moreover, the learning curve

[4] It happens that a number of the above individual organisms had their trials carried far beyond their learning asymptotes. The β_2's of five of these animals' $_s\dot{\bar{E}}_R$ differences (median N = 2016) were calculated. They were 3.0799; 3.7595; 3.8500; 4.9810; and 6.1974. These values suggest strongly that leptokurtosis may be the true basis of individual psychophysics and individual behavior theory generally, as well as of the pooled results by the Thurstone type of procedure. A repetition of Guilford's unique study (5) with 25 or 30 closely spaced weights and more subjects should yield a definite decision on this fundamental question.

yielded by the new quantification was substantially the same as that produced by the normal table except that the coefficient of the fitted equation was 10.6 per cent smaller.

11. From this corrected learning equation it is possible to derive all of the seven or so equations regarding important behavior functions (laws and constants) originally derived from the learning equation produced by the normal probability table.

12. By using a suitable adaptation of the above procedure we found that the standard deviation of the distribution of $_s\dot{\bar{E}}_R$ differences from consecutive trials during learning gradually increased throughout the first 7 to 9 trials from about 40 to nearly 90 per cent of that at the limit of learning. Later, undulations followed but according to no simple law.

13. We therefore conclude that:

A. Our data, and those of others, show that the paired comparison procedure when pooled from many subjects presents a characteristic deviation from the normal probability integral, known technically as a leptokurtic distribution.

B. The two contrasted distributions are sufficiently similar for the normal table, in conjunction with Thurstone's method of its use, to yield an empirical probability table which can be used to produce what purports to be a true quantification of momentary reaction potential ($_s\dot{\bar{E}}_R$) from leptokurtic paired-comparison data.

C. This quantification leads to a molar law of primitive learning.

D. This law of learning, in conjunction with other empirical circumstances, leads to the quantitative statement of seven other behavior functions.

E. Thus the method of the quantification of $_s\dot{\bar{E}}_R$ finally evolved is both fertile and consistent with known facts and principles.

F. The procedure followed in the present pioneering study should be applicable to the quantification of wide ranges of behavior beyond this sample.

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